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EDITED BY

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RAYMOND DODGE, YALE UNIVERSITY (*Monographs*)
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Journal of
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THE CUTANEOUS PERCEPTIONS OF SOFTNESS AND HARDNESS

BY ALICE H. SULLIVAN

University of Illinois

The purpose of this study is the analysis and synthesis of the cutaneous perceptions of softness and hardness. Upon these two perceptions very little experimental work has been done. Zigler briefly states, in connection with his study of clamminess, that softness is 'an integration of three attributes of pressure, viz. extent, intensity, and duration.' He further notes that softness 'lacks a definite peripheral contour.' [1]

Katz considers the hard-soft series (*hart-weich*) in a general study of touch experiences. Lateral movement is essential in producing these experiences, according to Katz; but the movement does not enter into the subjective experience.[2]

Our experiment falls into five parts: (1) analysis and (2) synthesis of the perceptions of softness and hardness with areal stimulation; (3) a study of the temperature qualities and their effect upon the perceptions of softness and hardness; (4) the effect of intensity of pressure upon the perceptions of softness and hardness; (5) the rôle of extensity in the perceptions; *i.e.* the relative merits of punctiform and areal stimulation, with a determination of limens for the perceptions. Part five, being as yet incomplete, will be only briefly

[1] M. J. Zigler, An experimental study of the perception of clamminess, this *Journal*, 1923, 34, 550 ff.

[2] D. Katz, Der Aufbau der Tastwelt, *Zsch. f. Psychol. u. Physiol. d. Sinnesorg.: Ergänzungsband* 11, 1925. See also a review of Katz by M. J. Zigler, *Psychol. Bull.*, 1926, 23, 327 ff.

mentioned in this study, and further results will follow in a later publication.

Experiment I; Analysis

As stimuli for the perception of softness, we used wads of cotton and bits of velvet: as stimuli for the perception of hardness, we used metal and wooden cylinders. The Os were blindfolded and the stimuli were applied manually to the back of the hand and, at times, to the forearm.[3] The instructions read as follows:

"After a ready signal, you will experience a cutaneous perception. Please report upon the experience in terms of quality, intensity, extent, and pattern (arrangement of the attributes). Then name the perception, if you can."

The analyses showed that the perceptions of softness and hardness have, at least, two compulsory conditions; namely, a specific patterning of qualities within the area stimulated, and a specific kind of boundary. We shall call these internal pattern and boundary line. Perceptions of softness are conditioned by an irregular pattern of pressures and an indefinite boundary line.[4] Perceptions of hardness are conditioned by a regular and even pattern of pressures and a definite boundary line. The following Tables (I and II) summarize these results. Table I shows the number of times an irregular

Table I

Internal Pattern

	Irregular pattern		Regular pattern	
Obs.	Hard	Soft	Hard	Soft
C	3	6	18	2
G	1	6	5	1
M	0	6	17	3
S	0	4	4	0
Y	0	17	13	6
Total	4	39	57	12

pattern of pressures was correlated with the perception of

[3] Five Os took part in the experiment, Dr. E. A. Culler (C.), Dr. C. R. Griffith (G.), Dr. E. F. Möller (M.), Dr. P. T. Young (Y.), and the writer (S.), all of them members of the Department of Psychology at Illinois. When S observed, a senior major in the Department acted as experimenter.

[4] This is, of course, the same as Zigler's 'lack of definite peripheral contour.'

softness, as well as the number of times a regular and uniform pattern of pressures was correlated with the perception of hardness. Table II shows the number of times an indefinite boundary line was accompanied by the meaning of softness and definite boundary line by the meaning of hardness.

A study of Table I shows that of 43 reports of irregular pattern of pressures, 39 of them were correlated with perceptions of softness, and only 4 with perceptions of hardness. 3 of the 4 were explained as being due to very sharp gradients of pressure, making a perception of a soft object impossible. Of 69 reports of regular pattern of pressure, 57 were correlated with perceptions of hardness, and only 12 with perceptions of softness. 3 cases of the 12 were explained as due to the fact that the object was perceived as wet, like gelatine or mercury.

Excerpts from the reports of the Os, bearing upon this condition of internal pattern follow:

(C.) A soft object like felt. The pressure is slightly uneven, and yet the pressure gradients are gentle and not abrupt and sharp as they would be if it were a hard object.

(C.) Soft felt. The slight irregularities in the pressures seem to be associated with softness.

(C.) A soft object. The pressure was not wholly uniform; I perceived it at different points with slight gaps between. Smooth and hard go together and soft and rough.

(M.) A little strip of pressure to which another pressure was added. The little block that appeared first was grainy and slightly irregular and meant something soft and wadded up.

(M.) A hard object. There was a pressure to which a cool was added. The pressure was hard and unyielding and very smooth. It meant a piece of metal.

(M.) The object was hard in places; but it was soft where it was rough. The roughness itself is soft.

(S.) Warmth and pressure of medium intensity. The pressure is unevenly patterned throughout the extent. It is soft.

(S.) Cold and pressure, both quite intense. The pattern of pressure shows even distribution and the boundary line is very definite. It is something hard.

(Y.) Pressure and no temperature. The pressure is not sharply defined. It is between smooth and rough. The pattern is not uniform; there are slight gradations in the intensity of the pressure. It is a soft piece of cloth.

(Y.) A hard object. The pattern of pressure is uniform and the boundary line is sharply defined.

Table II shows that hardness was always correlated with a definite boundary and softness with an indefinite boundary line. Boundary line appears to be even more compulsory than internal pattern.

TABLE II

BOUNDARY LINE

Obs.	Indefinite Boundary Hard	Indefinite Boundary Soft	Definite Boundary Hard	Definite Boundary Soft
C	0	7	11	0
G	0	3	0	0
M	0	2	0	0
S	0	3	4	0
Y	0	23	9	0
Total	0	38	24	0

The following descriptions are typical:

(C.) In softness, there is always a fringe of pressure around the outside.

(G.) A group of pressures, not very closely integrated; an irregular fringe of pressures around a core of more closely integrated pressures. It is cloth or felt. The meaning is given by the fringe of pressures around the core.

(M.) That is soft; it is like a film color; you can't say where it ends.

(S.) Soft. The pressure was even and uniform at the core, but it diminished in intensity toward the outside and had no definite boundary.

(Y.) Soft. It is just a splash of pressure with no boundary line. It goes off into nothing like the outside of the field of vision.

(C.) The line of demarcation is sharper in hardness than in softness.

(S.) Hard like metal. The pressure was uniform and the boundary line was definite.

(Y.) In softness, there are weak pressures which are spread out; but in hardness there are stronger pressures, more concentrated and more sharply delimited.

Summary

We see, then, that perceptions of softness are made up of irregular pressures which shade off gradually into nothing; while perceptions of hardness are made up of compact regular pressures of definite boundary line.

EXPERIMENT II; SYNTHESIS

We next tried to produce synthetic perceptions of softness and hardness by reproducing artificially the two compulsory conditions of internal pattern and boundary line. For stimuli, we used two wooden cylinders, one smooth on the bottom and one carved out, so as to give uneven bits of pressure. The weight of each cylinder was five grams.

Using as stimulus the smooth cylinder, we performed, in all, 104 experiments. Of these, there were 71 perceptions of hardness reported and 33 perceptions of softness. We per-

formed 100 experiments with the rough cylinder, 66 of them resulting in perceptions of hardness and 34 in perceptions of softness. We seemed to be placing a premium upon hardness; for the rough cylinder, after all, was just as likely to give prick as pressure, and whenever prick was experienced the meaning attached was never that of softness.

We next reversed our stimuli, using objects essentially soft. We wadded up pieces of cotton in such a way that one piece was smooth and even on the bottom, and the other piece was lumpy and irregular. Of 20 experiments with smooth cotton, 13 were perceived as hard, 7 as soft; of 20 experiments with lumpy cotton, 15 were perceived as soft, and 5 as hard.

This was not a very successful synthesis and the reason for the lack of success was only too apparent. It was impossible to control the temperature of the stimuli; for with unavoidable changes in room temperature we found reports of temperature constantly creeping into the O's reports. We began to see that the same stimulus which gave a perception of softness when warm would give a perception of hardness when cold. We had been convinced, to begin with, that reports of the effect of temperature upon the perceptions were due to the confusion on the part of the Os of true meaning and association. Very hard objects, such as metals, are most generally cold; and soft objects, such as cotton or wool, are slightly warm. We were now forced to admit, however, that mere change of temperature could upset completely our artificially produced conditions of internal pattern and boundary line. To be sure, we still found in our synthetic experiments regular pressure and definite boundary lines, whenever reported, being correlated with hardness, and irregular pressures and indefinite boundary lines being correlated with softness. But temperature appeared to have the effect of completely changing both pattern and boundary. It was necessary, then, to turn to a more complete investigation of temperature and its relation to the perceptions of softness and hardness.

Experiment III; The Effect of Temperature upon the Perceptions of Softness and Hardness

Here we began with the same stimuli as before; but we first warmed or cooled the stimuli in dishes surrounded with hot sand or ice. We found the smooth and rough cylinders satisfactory, but realized the need of a soft object which would exactly duplicate the size and pattern of the cylinders. Wads of cotton would not do. After much trial we found the ideal stimulus to be a small bag made of china silk and filled with cornstarch to give it body. The bottom of the bag was made of a circular piece of silk equal in diameter to that of the cylinders. China silk is the softest of silks and should serve as a perfect check upon the experiments with the hard wooden cylinders. In the one case, we could warm and cool the softest of objects and test out the effect of temperature in making it mean hard or soft; in the other case, we could warm or cool one of the hardest objects and again test out the effect of temperature.

We found that temperature as a condition is even more important than internal pattern or boundary line, for it can actually change both of these conditions. Hard objects can be made soft, in perception, simply by warming them. Likewise, soft objects can be made hard, in perception, simply by cooling them. Table III summarizes these results.

Table III

Temperature [5]

		Smooth wooden cylinder				Rough wooden cylinder				Silk bag			
	No.	Cold		Warm		Cold		Warm		Cold		Warm	
Os	Exps.	H	S	H	S	H	S	H	S	H	S	H	S
C	50	10			6	8		7	3	7		1	8
G	16	3			1	1			1	3	1	2	4
M	34	11	1	7	6	3	1		1		1		3
S	43	8		3	6	7			8	6			5
Y	34	9		2	1	1			5	3	2	2	9
Total	177	41	1	12	20	20	1	7	18	19	4	5	29

[5] H = hard; S = soft. The experiments for different Os are not equal in number. Some Os, at times, did not get perceptions of hard or soft. We then did not count the experiment. We wished to be, at all times, extremely careful not to suggest meanings to our Os; and if they failed to report softness or hardness, we did not ask any questions.

A survey of the Table shows that the smooth wooden cylinder, when cold, gave meanings of *hardness* 41 times and meanings of softness only once. The same cylinder, when warmed, gave meanings of *softness* 20 times and meanings of hardness 12 times. The rough wooden cylinder, when cooled, gave 20 reports of *hardness* and 1 of softness; when warmed, 18 reports of *softness* and 7 reports of hardness. The silk bag, when cooled, was perceived as *hard* 19 times and as soft 4 times; when warmed, the same bag was perceived as *soft* 29 times and as hard only 5 times. Out of a total of 177 experiments, we obtained 67 warm 'softs' as opposed to 6 cold 'softs' and 80 cold 'hards' as opposed to 24 warm 'hards.' This shows that warm 'softs' and cold 'hards' are the rule. The cases of cold 'softs' are so few as to be almost negligible. The warm 'hards' are more numerous. It seems to be difficult, at times, to eliminate boundaries with warmth; Os often reported that they distinctly felt the edge of the object and knew it must be hard. Careful Os, however, reported in such cases that the object was partly hard and partly soft—soft where it was warmest.

Examples of descriptions follow:

C. (cool, smooth cylinder): Pronounced coolness; hard and smooth. I judged it to be smooth metal. If the object were soft, it would not be so cool or so smooth. There was no discernible difference in pressure over a considerable area.

C. (warm rough cylinder): Pressure and warmth. It was hard and rough. The roughness of a hard object is different from that of a soft one. In hardness, the roughness has sudden pressure gradients, while in softness there is a gentler transition.

G. (warm rough cylinder): Light pressure; faintest hint of warmth. The pressure is lumpy. It is a solid soft hybrid or solid powdery object.

M. (cold rough cylinder): At first just pressure, then an area of pressure and cold making a cold pressure. When it was just pressure, it was irregular in pattern and soft, like those yellow erasers. When the cold came in, it almost got wet for a moment; then it changed and it meant a piece of metal, hard, cold, smooth.

M. (warm smooth cylinder): First pressure, then temperature added and it became a warm pressure. In the middle, it was warmest and where it was warm it was soft. You'd have to say the darn thing was soft in the center. It was soft just as a film is. You could push way into it. Yet it was hard at the edges.

S. (warm smooth cylinder): Slight pressure; moderate warmth. Boundary very indistinct. Pressure is more intense in center and gradually shades off to nothing. Soft like a woolen cloth.

S. (cold smooth cylinder): Pressure and cold. The boundary line is definite and the pattern of pressure is evenly and uniformly distributed. It is hard like metal.

Y. (warm smooth cylinder): A warm pressure. There was a lump of pressure and no definite boundary line. It ends in nothing. Probably a wad of warm cotton.

Y. (cold smooth cylinder): Cold and pressure. The boundary is well defined. It is probably a piece of metal.

Description of experiences resulting from the silk bag filled with cornstarch follow. In every case we warmed the bag and took a report; then immediately afterwards we cooled the same bag and took another report.

C. (warm bag): Warmth and pressure. It is somewhat soft like chamois leather; not very smooth; there are rough little points projecting out farther than the rest of the pressure, making the pattern irregular.

C. (cold bag): Cold, hard, flat, smooth, and metallic. The coldness and the polished surface are closely related.

G. (warm bag): Warmth and pressure. It was soft like a powder puff.

G. (cold bag): Cold and pressure. It might have been a piece of light wood; it was hard.

M. (warm bag): Weak pressure and moderate warmth. Pressure was more intense in center and shaded off gradually. It was soft.

M. (cold bag): Coolness and moderate pressure making a cold pressure. The pressure was even and regular over the whole of the area. Hard like a square stamp of metal.

S. (warm bag): Pressure and warmth. The pressure is even and regular at the core and it gradually diminishes in intensity toward the outside. It really has no boundary line. It is nice and soft and fluffy; the most perfect perception of softness I have ever experienced.

S. (cold bag): Cold and pressure. The cold is very intense; the pressure is weak. It is a layer of cold with pressure under it. The pressure is uniform and the boundary is well defined. It is hard like ice.

Y. (warm bag): Warmth and pressure. The boundary line is not sharply delimited. It is soft just like a finger touching my hand.

Y. (cold bag): Cold and pressure, both intense and well defined. It is smooth and hard. I'd say it is a piece of chalk.

Summary

The tabulated results, as well as the reports of the Os, show clearly how quickly the whole meaning of the experience changes when the temperature is changed. Our best synthetic 'softs' and 'hards' were produced by using a soft silky stimulus which could be quickly cooled or warmed. Under these conditions, with only a few exceptions, coldness produced hardness and warmth produced softness.[6]

We determined next to study simple experiences of cold and warmth in order to discover why warm objects are perceived as soft and cold objects are perceived as hard.

[6] The possibility of changing the whole meaning of an experience by changing temperature recalls an earlier study in which we were able to produce synthetic semi-liquids and semi-solids by lowering the temperature of the water used as stimulus. See A. H. Sullivan, The perceptions of liquidity, semi-liquidity and solidity, *Amer. J. Psychol.*, 1923, 34, 531–541.

Experiment IV; A Description of the Temperature Qualities

We wished now to find a method of arousing simple warm and cold qualities unmixed with pressure so as to distinguish between the effects of pressure and temperature in our experiments on softness and hardness. To our knowledge analogous methods of arousing cold and warmth, without pressure, have never been worked out.[7] After much trial and error, we were able to perfect an apparatus which gave us analogous methods of arousing warmth and cold.

The completed apparatus consists mainly of two parts: (1) a current of air flowing through copper tubes surrounded by ice and salt; (2) a current of air flowing through a pyrex glass tube containing an electric coil. The air can be cooled down to 22° C and heated to 46° C. An inclined manometer is connected with each air-system so that readings may be taken and the pressure of air kept constant at all times. The flow of air at a constant and controllable temperature is provided by a closed five-gallon jar to which water is siphoned from a large reservoir and from which it is siphoned to a like closed jar set at a lower level. By adjusting stop-cocks in this system, water flows at a constant rate into the lower jar from which air is driven through the double pneumatic system to the warm or cold jets emitting air to the dermal surfaces.[8] The manometric tubes have a scale unit of .01 inch of water. They afford a delicate measure of the air-blast at the skin. The air is dried before entering the double pneumatic system by passage through calcium chloride. The temperature of the electric coil is controlled by means of a lamp battery, and the coil is protected by asbestos coverings.

We had three of the regular Os (C., M., and S.) and added a fourth (B.), a senior student in psychology. Warm and cold spots were found and marked on the back or palm of the hand. The spots were then stimulated with warm or cold air. At the beginning of the experiment the pressure of the air was brought down to an intensity just weak enough to be sensed when a temperature spot was stimulated but not sensed over a pressure spot. This ranged between 0.4 and 0.65 inches on the manometric scale. The temperature for cold ranged between 23° and 26° C, and for warm, between 38° and 41° C. The instructions read, "After a ready signal, a cold (or warm) spot will be stimulated. Please give a complete phenomenological description of the experience." The stimulator was lowered to a distance of about 2 mm above a warm or cold spot and allowed to remain while the O gave his report.

There were in all 127 reports, 71 describing warmth qualities and 56 describing cold. These were distributed as follows among the four Os:

[7] For attempts to stimulate warm and cold spots without mechanical stimulation, see Alrutz, On the temperature senses, *Mind*, 1897, 6, 445. See also Dimmick's electric thermæsthesiometer for arousing warmth without pressure, *Amer. J. Psychol.*, 1915.

[8] Professor Bentley devised and set up the siphon system and arranged the manometers. The manometers were lent by Professor A. C. Willard, Head of the Department of Mechanical Engineering.

TABLE IV

Obs.	Warm	Cold
B	9	8
C	12	9
M	15	18
S	35	21
Total	71	56

Excerpts from the reports of Os follow:

Cold.

(B.) It is like a shaft of ice piercing down beneath the skin. It is very limited in extent and quite intense. It is compact.

(C.) It seems harder than warmth. The cold has a core or center which is well localized in one point; there is a kind of fringe of cold around this. The core is like a solid plug that extends down one or two millimeters, and the fringe is flat like a metal disc laid on the skin.

(M.) Cold; small; it pushes down. Not a pressury downness, but seems to be moving down. Very compact and hard and brittle. I mean by that that there is something metallic about it. It is sparkly.

(M.) Small and cold; always moving down in a thick medium, but it never really gets any place. There is a hard core and I get the impression that it is hard and definitely tri-dimensional, like a bullet. You could drive it into something. Then trailing out from this bullet or core is a film; it is like a comet; a trail of dust behind it.

(M.) The colder it gets the harder it gets. It sort of gets surfacy, but never gets to be a real surface. It is about as big as a grain of sand and it travels down five or six times its own size, always downward. It is always in three dimensions.

(S.) Like a pin-point of ice piercing down into the skin. It is a very tiny extent on the surface of the skin, but it goes down comparatively far beneath the skin. It is compact, and rather definite in boundary, although there are some radiations out from the central core.

(S.) A little compact shaft of cold, cutting downward, much longer than wide, and the length of the shaft pierces downward beneath the skin to the subcutaneous tissues. It is bright and bluish.

Warmth.

(B.) It radiates out over a considerable area; but it is always on the surface or near the surface. It gives a silky soft effect, like brushing something over the hand. There is no definite boundary to it and it is very weak in intensity. It is just like a silk covering on the hand.

(C.) It gets warmer and warmer; it comes in surges, sometimes more intensive than others. Not clearly localized except where pain shoots up. Whereas cold seems to be harder, like a layer along the surface, this is softer and looser and thicker than the fringe of cold. Of course it is not deeper than the core of cold which extends down into the skin. In warmth the slight sting seems to shoot up or well up as though the whole mass were somewhat soft and loose and bubbling up.

(M.) Warm. It is a film that is very thick; in constant motion up. It is not a uniform film either, it is made up of clouds and it billows; never any spaces but just big puffs of film. It is deep. You never do get to the bottom of it. (Cold is very definite; you can almost put your finger on it and say, "This is a little shaft of cold.")

You could never find this; if you put your finger on it, it would be *in* it. It is dark, dull, blunt; while cold is keen as if it were a knife that would cut, and is alive and sparkling.

(M.) Warm and dark and dull. An indefinite cloud, doesn't seem to have any boundaries. This is big and thick and deep; no doubt about its being a film. Like all films, you have three dimensions by implication, but don't see surfaces to get them by. It is full of tiny particles; they are close together; but there is room enough for them to move up, the way steam comes off a kettle.

(S.) Quite intense warmth. Core is mixed with pain and stabs downward. Around the core there is a fringe of weak diffuse warmth, indefinite in boundary and filmy. It is a very thin film, like a little soft veil over the hand. It is a tannish orange color.

(S.) Warm, intense at core; less intense around the core; no boundary; it simply fades off into nothing. It is almost flat, but has a hint of thickness about it. It is like a piece of heavy velvet that has been just put under the skin.

(S.) Warm, moderately intense. A large patch of it. It is grainy, very much like heat waves as seen in summer, only these grains are not moving. It is almost non-localizable and has no boundaries. There is a suggestion of oiliness about it, very much like a feather dipped in oil and laid under and on the skin. There seem to be *two* kinds of warmth, a less intense warmth which is silky and soft and like a veil, and a more intense warmth that is smooth and thick like the touch of feathers dipped in oil.

Summary

We found that warmth is not a single quality any more than pressure is. Just as there are contact and neutral or solid pressures, so there seem to be three distinct qualities of warmth: light warmth, smooth warmth, and stinging warmth. On the other hand, there seems to be just one quality of cold. We found the following phenomenal characteristics for warmth; diffuse, indefinite in boundary, between two and three dimensions, large, billowing up from just below the surface, dull, cloud-like, and filmy. We found the following phenomenal characteristics for cold: compact, definitely bounded, more tri- than bi-dimensional, small on surface of skin, but piercing downward far below the surface, bright, metallic, and 'surfacy.' The two qualities are differently localized; warmth is on the surface or just beneath, cold begins on the surface and extends downward almost to the subcutaneous tissues. This localization, however, for both experiences is very indefinite. One O (S.) is synæsthetic and for her one of the main differences between warm and cold is that of color. Warm is tannish orange, becoming reddish orange at high intensities; cold is a silvery blue.

The results of our descriptions of warm and cold disagree, in part, with those of other experimenters. Alston, for instance, says that cold is referred to the surface and warmth wells up from beneath.[9] We found cold always piercing down from the surface to a comparatively great distance beneath, while warmth does, at times, well up from beneath the surface, and at other times simply remains just beneath the surface, or on the surface. We cannot wholly agree with Knight when she says that "warmth, particularly a warmth of good intensity, is qualitatively more like sting than Cutolo has led us to suppose." [10] We found stingy warmth only at very high intensities, and true warmth has no sting in it. Bershansky, when she found a compact cold, explained it as being due to the pressure quality.[11] We found compact cold without any pressure.

The qualities of warmth and cold are more like pressure than like any other touch quality. Warmth is somewhat like contact; cold is more like neutral or granular pressure. Warm, cold, and pressure are somehow interrelated; they are not independent of each other, hence one may get the same effect from change of temperature as from an intensive change of pressure.

The descriptions of the warm and cold qualities explain why warm objects are perceived as soft and cold objects are perceived as hard. Warm itself has the pattern, texture, and lack of boundary which we find in all perceptions of softness; it is diffuse, filmy, and indefinite in boundary. Cold itself has the pattern, texture, and boundary which we find in perceptions of hardness; it is compact, uniform, and definitely bounded. Warms and colds alone never give true perceptions of hard and soft for they are pre-perceptive, non-objective, and non-localizable; while the true meanings of soft and hard are perceptive, objective, and localizable. But warmths fused with pressures, as given in areal stimulation, will obscure the pattern and boundary of the pressures and

[9] J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *Amer. J. Psychol.*, 1920, 31, 303.

[10] L. Knight, The integration of warmth and pain, *ibid.*, 1922, 33, 587 ff.

[11] I. Bershansky, Thunberg's illusion, *ibid.*, 1923, 34, 295 ff.

make the whole experience into something diffuse and soft. So will cold fused with pressures increase their apparent depth, and emphasize boundaries, thus facilitating an experience of hardness.

Katz also found that temperature facilitates the recognition of certain materials and objects. He says that metal, glass, and oilcloth are characteristically cool, while wool is warm. He found cool to be more helpful to the recognition of metals and glass than warm is to the recognition of wool. He explains this as due to memory rather than to any characteristic of the temperature qualities themselves.[12] We have found that the characteristics of the temperature qualities themselves will explain better this phenomenon.

Experiment V; Quantitative Factors

1. Intensity

We had expected intensity to play an important part in the perceptions of softness and hardness; yet the Os rarely mentioned it in their reports. In order to be certain about this point, we next planned a series of experiments in which wooden cylinders of known weight were used as stimuli. The weights ranged from 5 to 95 grams. There were 3 Os in this experiment; C., M., and S. The method of experimentation and the instructions were the same as in the first three experiments.

We found that intensity of pressure is not a compulsory condition of the perceptions of softness and hardness. Weak intensities may give hardness or softness; so may strong intensities. The following table and quotations from the reports of the Os bear out this conclusion.

Table V

Intensity

	No. of Exps.	5 gr. H	5 gr. S	10 gr. H	10 gr. S	20 gr. H	20 gr. S	40 gr. H	40 gr. S	95 gr. H	95 gr. S
C	26	10	6	0	0	1	1	2	1	1	4
M	32	2	4	2	3	2	4	1	4	4	6
S	30	0	4	0	6	0	6	0	3	5	6
Total	88	12	14	2	9	3	11	3	8	10	16

[12] *Op. cit.*, p. 334.

The Table summarizes 88 experiments. Of these, there were 30 perceptions of hardness and 58 perceptions of softness. 12 perceptions of hardness resulted from the lightest weight, that of 5 grams, and 10 perceptions of hardness resulted from the heaviest weight, that of 95 grams. This shows that hardness is perceived as well from light stimuli which give weak pressure as from the heavier ones. The same may be said for softness. There were, in all, 58 perceptions of softness, 14 of these resulting from stimulation with the lightest weight and 16 of them resulting from stimulation with the heaviest weight.

The following reports were typical.

C. (cylinder, 95 gr.): Felt soft on the bottom; but it was too heavy to be a piece of rubber. The pressure gradient along the edges was gradual. Also, it was warmish and these two factors made me think it was of soft texture. It was heavier, though, than I'd expect from a soft object. Maybe it was some kind of metal on top of chamois or soft cloth. *I had the impression that it was too soft for its weight.*

M. (cylinder, 95 gr.): A tickle first of very small extent. It increased in extent. Uniform pressure and cool. It wasn't cold enough to be hard like stone, and there was a little graininess in it. It was as if there was a layer of something soft and stringy there and it meant a *piece of soft cloth over metal and the metal was pushing it down.*

M. (cylinder, 5 gr.): Begins as a pressure; soft and all wadded up. Then it begins to get cool and as it gets cool, it gets stiff. This one never did get really hard like metal, but it was like cardboard.

M. (cylinder, 5 gr.): A small bit of pressure, close to tickle and lively. That was lost and cold and pressure came together, uniform, moderately intense, smooth, hard. The intensity in this experience was the same as the intensities of ones that were soft; one would have had to say this thing wasn't hard, but it *was. It meant some kind of metal that you were holding lightly on the skin.*

S. (cylinder, 95 gr.): Warm, heavy, and soft. Medium duration and intensity. The pressure was indefinite in boundary. It felt like velvet which had been padded to make it heavy.

S. (cylinder, 40 gr.): An even pressure over a rather small area; there were small slight bumps in this even pressure. It was rather soft, like a piece of undressed kid.

Summary

The distribution of 'softs' and 'hards' in the table, as well as the reports of the Os, prove rather conclusively that intensity of pressure is not a compulsory condition of the perceptions of hardness and softness.

Extensity

We are now engaged in a series of experiments to determine limens of extent for softness and hardness. From our experiments on warm and cold spots, we think it safe to conclude that punctiform stimulation will not give true perceptions of softness and hardness. We found warmth ideal for perceptions of softness, yet a simple quality of warmth is not, in an objective sense, really soft; it is rather 'pre-soft.' Cold is ideal for perceptions of hardness, yet a simple quality of cold is not really hard, in an objective sense; it is 'pre-hard.' A later study will consider the matter of extensity more fully.

Temporal Factors

It will be recalled that Katz considers lateral movement necessary for perceptions of softness and hardness. Our reports fail to substantiate this. Naturally some movement was involved in applying the stimulus, but this was very rarely perceived by the Os. Often, too, a perception of hardness would change to one of softness *after* the stimulus had been at rest on the hand for several minutes. Only two Os mentioned the temporal aspect at all. Their reports follow.

(M.) I felt the object come to rest on the hand; but the temporal aspect seemed to have nothing to do with the meaning.

(C.) I do not believe the sudden increase in intensity as the stimulus is applied has anything to do with the perception. It is perceived as soft just as well now when it is at rest on the skin.

Conclusions

There are varying degrees of hardness and softness, and for this reason it is difficult to state just what the compulsory conditions are in all cases. The optimal hardness, however, depends upon the following conditions: (1) pressure of the same intensity throughout the area stimulated; (2) a well-defined boundary line; (3) coldness. The optimal softness is conditioned by: (1) pressure of varying intensities throughout the area stimulated; (2) an indefinite boundary line; (3) warmth. It is sometimes possible to experience a warm 'hard'; but it is almost impossible to experience a cold 'soft'

unless wetness or moisture enters in. The warm 'hards' are experienced only when the internal pattern and well-defined boundary are peculiarly clear, in which case the warmth cannot obliterate these. Even in such rare cases, careful observers often detect a softness within the hardness when warmth is apparent. Intensity of pressure is not a compulsory condition of the two perceptions. Extensity, in the sense of being areal rather than punctiform, appears to be compulsory. Movement is not necessary to the perceptions of softness and hardness.

STUDIES IN PSYCHOMETRIC THEORY

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XIV. On the Probable Error of the Limen (Method of Constant Stimuli)

In this article we aim to propose a simple and adequate procedure for determining the probable error of the Urban limen, that is, the median of the cumulative percentile function known as $\Phi(\gamma)$.

A constant-stimulus limen may properly be called the 'standard' measure of differential capacity in the organism; and yet all existing means for ascertaining the variance of this important magnitude are alike unsatisfactory, being either inadequate or too laborious for general use. The original derivation by Urban (*12*, xvi, 226; *cf.* *5*), to whose careful and critical labor is due the method of constant stimuli in its present form, proved to be erroneous. Thomson's solution (*11*), though both in conception and in execution sound, is regrettably irksome in practise. Boring (*1*) had the simple and useful device of computing p.e. from h, but introduced a mode of weighting that can hardly be accepted. Culler (*3*, vi) also deduced p.e. directly from h, but used a value for N (whole number of reports on which the limen is based) that is true only for certain limiting conditions. In the present article therefore we mean to generalize our former proposal.

Let S and J be two stimuli (weights) presented to an observer under stated conditions; let Obs. respond to J upon each presentation in one of three ways (Heavier, Doubtful, Lighter); let p_H be the proportion of 'heavier' in the whole number of reports elicited by a given J. As J increases p_H will rise between .00 and 1.00, at first slowly then more rapidly then again slowly, roughly describing in its course a curve

known as the psychometric function $\Phi(\gamma)$. Let a be median of the ogive which best fits (by least squares) the ascending series of H-percentages and let $h(= 1/\sigma\sqrt{2})$ be the 'precision' of their distribution. The h-method, as formerly proposed, was meant to give an exact p.e. for the approximate (fitted) values of p_H as against Thomson's procedure in finding an approximate p.e. for the exact (observed) p_H. We used the conventional formula for p.e. of a median in a normal series of frequencies:

$$\text{p.e. } (Md) = \frac{.84535 \text{ s.d.}}{\sqrt{N}} = \frac{.84535}{h\sqrt{2N}}.$$

The formula is defective because no means are proposed for ascertaining the true value of N. The more ordinates (p_H) are known, the more reliable is the median which they define; seven values of p_H give a more reliable threshold than will two. Instead of being a fixed quantity, as we previously assumed, N will vary with the number and magnitude of p_H. Consider

TABLE I

J (1)	n_H (2)	p_H (3)	P (4)	$P \times n_H$ (5)	$P \times p_H$ (6)	f (7)	$f \times 6$ (8)
84	1	.00	.00	.00	.0000	1	.0000
88	9	.02	.19	1.71	.0038	2	.0076
92	40	.09	.51	20.40	.0459	3	.1377
96	100	.22	.80	80.00	.1760	4	.7040
100	186	.41	.98	182.28	.4018	3	1.2054
104	403	.90	.54	217.62	.4860	2	.9720
108	423	.94	.40	169.20	.3760	1	.3760
	1162	2.58		671.21	1.4895	16	3.4027

J = the successive comparison-stimuli; n_H = the number of 'heavier' in 450 reports; $p_H = n_H/450$; P = Urban weight of corresponding p_H. The figures in tiers 7–8 are wholly fictitious and are included merely to illustrate a mode of computation; f = relative frequency (hypothetical) of the several J; tier 8 = tier 6 multiplied by f.

Table I, the materials of which are taken from Obs. I, S_2 (*12*, xv, 287, Tab. 11); S_0 (standard) = 100 grams, n = 450 (each J is compared with S_0 450 times; not to be confused with N). If all reports were of equal (unitary) value, the whole number of cases on which this curve rests would clearly be 1162 ($= \Sigma n_H$) or 2.58 × 450. But the p_H have unlike precision (reliability) and therefore unequal weight (P). Upon multi-

plying each n by P, we have the net contribution of those reports to the final curve. In row 2 ($J = 88$) 'heavier' appeared nine times in 450 reports; these nine weigh but little in the Urban-schema ($P = .19$), having no more significance than would 1.71 ($= 9 \times .19$) cases of unit-value. These 1162 ($= \Sigma n_H$) cases of unequal precision, none exceeding unity, reduce then to 671.21 ($= N$) reports of unit-reliability. The true value of N (number of reports on which the curve is based) should be 671 instead of 450, which was used in the other paper.[1]

With this change our formula [2] becomes

$$\text{p.e.}\ (Md) = \frac{.84535}{h\sqrt{2\Sigma nPp}},$$

the added factor being ΣPp, which was formerly taken as unity in all cases. With five or more comparison-stimuli equally disposed, this factor (ΣPp) will usually be greater than one; with less than five it will often be smaller. Its magnitude further depends upon the proportion of extreme (heavier) reports for the stimuli used; but in any event ΣPp may be called one only when an appreciable margin of error is tolerable.

We now impose the final test: what pragmatic sanction has the formula? Does it foretell with usable precision the actual dispersion of the limens?

From Table II we may find how well our h-values agree with the actual disposition of the fractional limens; the way these partial limens fall sets a highly useful norm for the validity of any proposed formula. With Obs. I–VII the true p.e. (in 1, 2) are uniformly higher than the h-figures (in 5, 6); with II*b* and VIII they are much the same or a trifle lower. The relation of each pair may be taken at a glance from the proper ratios (in 7–10); they extend, in the case of p.e. (Md),

[1] The percentages p_H are commonly more convenient to use than are the actual numbers n_H. Here we have $\Sigma P_p = 1.4895$, which multiplied by 450 gives 670.28 for the net area of our psychometric curve, as against 671.21.

[2] We here assume that n (number of reports upon a given J) varies from one J to another. When, as in the example above, n is always the same (450), it may be set before the summation-sign (Σ).

from 0.55 to 3.92. From these figures it would seem on first view that we cannot forecast with any real assurance the actual variance of our sub-limens; but two facts soon appear: (*a*) with Obs. II*b* and VIII theory and fact agree far better than with I–VII; there 13 out of 16 ratios (81 per cent.),

Table II

	Actual Dispersion				Predicted Dispersion		Ratios				Coefficient of	
	p.e.(Md)		$Q/\sqrt{n}$		*h*-method		1/5	2/6	3/5	4/6	Divergency	
	S_1	S_2	S_1	S_2	S_1	S_2	S_1	S_2	S_1	S_2	S_1	S_2
Column no.	1	2	3	4	5	6	7	8	9	10	11	12
I	.328	.325	.254	.243	.162	.172	2.02	1.89	1.56	1.41	1.37	1.44
II	.268	.248	.234	.182	.177	.182	1.51	1.36	1.32	1.00	1.21	1.22
III	.574	.375	.462	.340	.150	.160	3.83	2.34	3.08	2.12	2.09	1.78
IV	.479	.460	.437	.416	.199	.209	2.41	2.20	2.20	1.99	1.53	1.50
V	.569	.792	.412	.845	.206	.202	2.76	3.92	2.00	4.18	1.75	2.14
VI	.261	.465	.179	.439	.215	.227	1.21	2.05	.83	1.93	1.38	1.81
VII	.357	.470	.355	.469	.202	.222	1.77	2.12	1.76	2.11	1.24	1.43
II*b* i	.269	.218	.234	.128	.207	.238	1.30	.92	1.13	.54	1.07	.86
ii	.208	.187	.212	.205	.206	.234	1.01	.80	1.03	.88	1.01	1.08
iii	.225	.135	.212	.160	.216	.228	1.04	.59	.98	.70	.87	.82
iv	.203	.158	.187	.081	.205	.227	.99	.70	.91	.36	.98	.99
VIII i	.322	.289	.258	.302	.216	.254	1.49	1.14	1.19	1.19	1.27	1.23
ii	.176	.152	.183	.164	.212	.205	.83	.74	.86	.80	.93	1.11
iii	.145	.128	.102	.104	.208	.233	.70	.55	.49	.45	.72	.86
iv	.358	.291	.304	.253	.207	.241	1.73	1.21	1.47	1.05	1.38	1.10

The "actual dispersion" is taken from the distribution of partial or fractional l imens. Urban divided the reports from Obs. I–VII into series of 50 each: nine for I–III (450 in all) and six for IV–VII (300); see *12*, xv, 268–70 and 287. S_1 and S_2, h_1 and h_2 were computed for each division of 50 (*13*, 276–78). In like wise 28 series of 50 each from Obs. II*b* and VIII were laid into four quarters (by the writer) and the actual dispersion found within each set of seven (*13*, 297–8); for II*b* we use the last 28 rows (nos. 10–37), the first nine being taken from II*a*. The spread of these limens S_1 and S_2, is found in the usual way:

$$\text{p.e.(Md)} = \frac{.84535}{n}\sqrt{\Sigma d^2},$$

n being nine for I–III, six for IV–VII, seven for II*b* and VIII; thus we have the figures of col. 1, 2. In 3, 4 we have the quartile deviation (semi-interquartile range) of the same limens, divided by $\sqrt{n}$.[3] In 5, 6 are the several p.e. by our *h*-formula; for I–VII h_1 and h_2 are taken from *12*, xvi, 192, Tab. 43–44; for II*b* and VIII, an arithmetic mean of the seven *h* in each quarter is used (*13*, 297–8). In col. 7–10 are the successive ratios of actual to theoretical values (7 = col. 1 divided by col. 5, and so on). In 11, 12 are the coefficients of divergency or Lexian ratios, whose meaning and use will appear in the text.

[3] It is a well-known principle of mensuration that any measure varies directly in precision and reversely in error to the square root of the number of observations which the measure incorporates; so the conventional formulæ for variance of mean, median, s.d., coefficient of correlation, and so on (cf. *9*, 77). To make the quartile deviation comparable with p.e.(Md) we must therefore divide it by $\sqrt{n}$.

here only 2 in 14 (14 per cent.), lie within 2/3 and 3/2;[4] (*b*) the agreement is notably higher with observers of known competence and careful training (as II*a* and II*b*, Urban himself) than with others. The two-fold question is: Whence do the discrepancies arise? How may they be removed?

We have noted elsewhere (*4*, xi, 182) that Urban's data for Obs. I–III are by no means clear of practise-effects, and the same may be assumed of IV–VII; this being true, the sublimens are bound to scatter more than would otherwise be true. The coefficient of divergency, first proposed by Lexis and often called the Lexian criterion or ratio (L), is a useful device for probing the 'causal' factors by which any collective object (frequency-distribution) is determined and for testing whether and in what measure these factors are open to change (*9*, ch. VI; *10*, ch. VI). To get the meaning of L let us resort to the usual urn-schema. Given ten urns of white and black balls in differing proportions; a set (s) of 10 is drawn and the percentage of whites (p_w) recorded; n sets (a total of ns draws) are completed. Three typical procedures are open. (*i*) We may always draw from the same urn (identical composition throughout the series of ns trials); the n values of p_w thus drawn will, apart from casual irregularities, constitute a binomial or Bernouillian series $(p + q)^s$, whose Lexian ratio is unity and dispersion 'normal.'[5] (*ii*) We draw a set of 10 from urn 1, another set from urn 2, and so on (constant proportion within a given urn, but changing from set to set); the n values of p_w now form a Lexian series, wherein $L > 1$ and dispersion 'over-normal.' (*iii*) We make up a set of 10 by drawing one ball from each urn (unlike probabilities of white from draw to draw, but the same combination of probabilities within each set); these n values of p_w compose a

[4] That is, theoretical p.e. (by our formula) is higher by 50 per cent. than true p.e. (2/3) or true is 50 per cent. higher than theoretical (3/2).

[5] The reader will not confuse 'normal' dispersion with 'normal' curve. An urn whose composition holds from draw to draw will yield a normal dispersion of p_w, but the actual proportion of whites in the urn may of course vary from .00 to 1.00. A normal (gaussian) curve has normal dispersion as well; but many curves of normal divergency, such as $(.1 + .9)^s$, are by no means gaussian. Comparative judgments of difference are known (*4*, 289) however to approximate normality [$\Phi(\gamma)$] insofar as the observer's attitude and criteria become stable and uniform.

Poisson distribution, whose variance is lowest of all; $L < 1$ and dispersion 'under-normal.'

The Lexian ratios are computed in this way. Consider Fernberger's data as reported by Urban (*13*, 294–5). In the column for 88, kleiner, rows 1–7 (p. 295), are these percentages in order: .66, .74, .82, .72, .90, .92, .94. Their mean is .814 with an actual s.d. of .1019; the binomial s.d. (normal dispersion) for these figures would be $\sqrt{pq/s}$, where p is the mean value (.814), $q = 1 - p = .186$, $s = 50$ (trials in one set; each p is a given percentage of 50 reports). Solving, $\text{s.d.}_B = .0550$, whence the Lexian ratio (L) = .1019/.0550 or 1.850 with p.e. of .333.[6] We see that 1.000 differs from 1.850 by 2.55 p.e. If this difference may be taken as significant for present purposes, the seven percentages above compose a Lexian sequence; the likelihood of 'kleiner,' though fairly constant within a given set of 50 trials, varies widely from one set to another (note the range, 66–94). The shifts in performance during these seven periods are too extensive to be ascribed to 'chance' aberrations; the obs. is himself changing in attitude or criteria from time to time. Now consider rows 15–21, kleiner, 88, p. 294. The figures straggle much less: .84, .94, .88, .88, .88, .96, .82. L here comes out 1.005 ± .181; the percentages form a binomial series $(p + q)^6$, the factors which condition a given report being substantially identical for the seven sets; what fluctuations appear may properly be referred to 'chance' alone. Consider, finally, rows 15–21, kleiner, 88, p. 295, with p of .80, .78, .86, .78, .84, .84, .78; these scatter even less than before and $L = .577 \pm .104$; a departure from unity of 4.07 p.e. This may be called a Poisson series, wherein the likelihood of 'kleiner' varies indeed from trial to trial but holds from set to set. An obs. in judging may use three standards, differing in precision;[7] the first

[6] The probable error of L is commonly given as $\frac{.4769\,L}{\sqrt{n}}$, where n gives the number of sets, the number of separate values for p; 7 in our example. *Cf.* Rietz (*9*, 88) where n should replace s; also Charlier (*2*, 41 and 50) who uses the formula without stating it.

[7] Say, pressure on finger-tips (1), tendinous strain in hand (2), kinesthesis in forearm (3). These would be sure to differ in precision so that reports issuing from them would show unequal probabilities of 'kleiner.' Combined criteria may also arise (1 + 2). This manifold of criteria may well be compared to an array of urns with unlike proportions of black and white.

report may come, say, from criterion 1, the next from 2, then 3, then again 1. If he applies the same three in every set, but uses now one now another from trial to trial, he reproduces the Poisson-scheme; and his reports are more stable, less erratic than if he were confined to the same criterion throughout.

On first view the high stability of the Poisson-figures may be puzzling. To Lexis indeed (author of *L*) and many older authorities a truly normal dispersion was a rare thing in nature and a subnormal wholly impossible, save when the percentages were no longer 'free' but were governed instead by some inherent 'law' or external control. A regiment of guards, e.g., who are picked for tallness will naturally deviate less in height than does a 'normal' (unselected) group. Only when the individual measures (draws from Nature's urn) were thus culled over and assorted by special design could subnormal variance appear. So long as chance ruled with no let or hindrance, our distributions might indeed be overnormal, as the underlying causes ebbed and flowed despite our knowledge and control, or else normal when by rare good fortune they 'held still' until our measures were made; but never undernormal.[8] These older views are no longer tenable. Suppose we note the color in 10 draws from each of 10 urns, all differing in composition ($n = 10$, $s = 10$). If by a 'set' we mean 10 draws from a single urn, the ten values of p (white) will make a Lexian series (overnormal); if 'set' be defined as one draw from each urn, the ten p will form a Poisson series (undernormal). It does not seem that one way of grouping the same draws is any more 'bound' or 'free' than the other, unless 'freedom' be defined ad hoc as degree of variability; a Poisson-schema is no more due to arbitrary control than is the Lexian.

[8] Zizek asserts (*14*, 330): "Series of infra-normal dispersion . . . have up to this time not been discovered." "We may assume that such an infra-normal dispersion or supra-normal stability could be exhibited only by mass-phenomena governed by some exterior restricting force." Czuber (*6*, 321): Es kann "Massenerscheinungen, denen der Charakter des Zufälligen zukommt, mit anderer als normaler oder übernormaler Dispersion nicht geben." "Vielmehr müsste angenommen werden, dass durch einen absichtlichen Eingriff, eine Norm, ein Gesetz, die Tendenz nach einem bestimmten Verlauf erzeugt werde; dadurch geht die Unabhängigkeit der Einzelfälle verloren . . ." For views of Lexis, see *7*, 80–81; of Blaschke, *7*, 82; of Kaufmann, *7*, 81.

It follows from all this that an observer's performance will be less uniform and precise if he is confined to a single mark or criterion of 'heavier' than if he works out and applies a whole set of them. Suppose two people differ in garb, gait, girth, stature and pigmentation; will they not be more surely and readily distinguished than if identical in all but stature alone? In like manner a multiplex of criteria for 'heavier' (in fingers, hand, forearm) seems to stabilize the judging process; the comparison is shored and guarded, as it were, on every side from extreme aberration; when one 'earmark' fails another may serve instead. Any difference-criterion, like all organic functions, is likely to vary a good deal in efficiency; but a set of three will at least be more stable than any one of the triad. We can now see more clearly the real nature of the so-called 'stimulus-attitude,' which has been deplored because it commonly uses more than one criterion in discriminative work. In a former study (art. x, *3*, 131) we tried to prove that this attitude "promotes discriminative efficiency and precision in the organism." The reason now is clear: it makes of the observer a Poisson-mechanism and thereby stabilizes observation and report. From the above analysis our former conclusion then is well confirmed; we may say with reasonable assurance that when an observer has, say, three active and well-defined criteria for 'heavier' of which any or all in combination may serve to guide the response, his reports will be more uniform and precise than when one criterion is his sole recourse.[9]

Having found the Lexian ratio for each comparison-stimulus in turn, we are prepared to get the mean coefficient for the whole lot of six or seven. Geometric mean is the logical average for a set of ratios, but arithmetic proves to be more useful in the regression-equation. Both $\Phi(\gamma)$ and the p.e.-formula above presume that the successive percentages of 'heavier' will rise at a normal rate (gaussian percentile curve); only if this be true is either procedure truly valid and pertinent. We may then expect a relation between the two ratios: L and actual p.e./predicted p.e. When L is unity, our p.e. by the

[9] Cf. Culler (*3*, x, 126–129).

formula should agree sensibly with the actual dispersion of the fractional limens; it should be too small when $L > 1$, too large when $L < 1$. We can readily test whether in fact this co-variation occurs. Do the ratios in col. 7, 8, (actual/predicted p.e.), correlate with the Lexian ratios? Here are the figures:

TABLE III

MEAN RATIOS

	N	r_{RL}	Arithmetic regression	r_{RL}	Geometric regression
S_1	15	.934	$R = 2.6\,L - 1.5$	.922	$R = 3.0\,L - 1.7$
S_2	15	.928	$R = 2.7\,L - 1.8$	.900	$R = 2.8\,L - 1.9$
$S_1 + S_2$	30	.918	$R = 2.6\,L - 1.6$	.889	$R = 2.8\,L - 1.7$
p.e. (est.)			.23		.26

r = correlation between R (actual p.e./predicted p.e.) and L (mean Lexian ratio, whether arithmetic or geometric; Table II shows only the former); N = number of measures; S_1 = limen for 'lighter,' S_2 for 'heavier'; p.e. (est.) = the probable error of any R calculated from equation for $S_1 + S_2$.

The geometric means are clearly inferior for present needs: (1) r is lower and p.e. (est.) higher; (2) when $L = 1$, R should also be unity (as in the arithmetic equation) instead of 1.1. Only the arithmetic ratios are considered from here on.

From the high correspondence of L and R (.918), the causal dependence of R upon L seems to be undeniable; if the actual p.e. (limen) exceeds or falls below our predicted h-value, the cause may properly be ascribed in major degree to a supra- or infra-normal dispersion (L). When L is sensibly unitary we need no corrective equation at all; but inasmuch as $\Phi(\gamma)$ and our formula both presume a gaussian array of percentages ('heavier'), the predicted p.e. cannot possibly be true and dependable if the general dispersion be markedly a-normal. Here is where the regression-equation enters; knowing the coefficient of divergency L, we can easily foretell the 'most probable' value for R and thus for p.e. The p.e. (est.) is pretty large; but the accord of theory and fact seems to be adequate for most practical needs. Of the corrected figures, e.g., all but four (87 per cent.) lie within 2/3 and 3/2 of the true p.e. and three of these four are due to one obs. (VIII) whose scores seem less consistent than the rest.

TABLE IV

RANGE OF RATIOS *R* (ACTUAL P.E./PREDICTED P.E.)

	8/9 to 9/8	4/5 to 5/4	2/3 to 3/2	1/2 to 2/1
Original	.13	.30	.50	.70
Corrected	.40	.63	.87	1.00

This table shows how well our predicted p.e. agree with actual figures. In first column we see that 13 per cent. of theoretical p.e. as directly computed from *h*-formula above lie within 8/9 to 9/8 of the true values; when corrected by means of regression-equation, 40 per cent. fall within this range.

The 'predictions' above are of course reflexive, since the actual scores we are trying to forecast have been used in the regression. The real question is: How well does our equation prognose the p.e. of a new observer? Upon computing the regression for these data, omitting the 8 scores of Obs. VIII altogether, we get: $r = .934$, $R = 2.7\,L - 1.7$, p.e. (est.) = .22. The two equations (with and without VIII) give these predicted values for VIII in order:

Including VIII	1.86	.89	.35	1.13	1.62	1.26	.66	1.28
Excluding VIII	1.89	.90	.33	1.14	1.65	1.27	.66	1.30

The new figures (without VIII) are virtually identical with the old, differing in no case by more than .03. Upon omitting both VIII and II*b*, using only the figures for I–VII, we get: $r = .898$, $R = 2.6\,L - 1.5$, p.e. (est.) = .23. The two equations (with and without VIII and II*b*) are again almost identical; and the predicted values differ in no case by more than .10. From the partial equation, derived only from the original Urban observers, we can forecast the scores of Fernberger's two (II*b* and VIII) almost as well as by means of the combined equation.[10]

When the observer is well-seasoned (free of practise-improvement) and holds to a single standard throughout, *L* will stay near to unity and we need no correction at all. The mean of 8 *L* for II*b* is virtually one (.992); in five cases the original p.e. taken directly from our *h*-formula is better than

[10] A period of five years (*13*, 293) separates the data for II*b* and II*a* (Urban); we have therefore treated them as distinct. Obs. VIII is far less uniform and dependable than say II*b* (*L* ranges from .75 to 1.33 as compared with II's range, .85 to 1.11) and tends to increase the p.e. (est.) of any equation to which he is admitted; his discrepancies should therefore not weigh too heavily upon us.

when corrected by regressive equation, and in three not so good; in VIII likewise, where the mean of L is 1.051, the ratio is four to four. When L is near unity then the corrective equation is not worth using at all. From novices who are likely to change in attitude and criteria from day to day we may expect an overnormal dispersion greater than our own p.e.; while for a competent observer who sustains and uses a multiplex of criteria (stimulus-attitude) our theoretical figure will be too high. In any event the h-formula will hardly be far astray when the data are sensibly gaussian, as they should be if we use $\Phi(\gamma)$ at all; only when markedly a-normal need the corrective equation be applied.

A few questions remain. (1) Are we justified in deriving and using a regressive equation from 30 cases? The reader will note that we are here dealing with quantities of much higher precision than ordinary single observations. Each of our 30 L is a mean of seven (or six) primary ratios, while the relation of predicted to actual p.e. is a quotient of two means, the latter representing 6–9 fractional limens, the other 400–1000 unit-observations. Our 30 figures then weigh a good deal more (conservatively 5×) than do ordinary single measures and accordingly seem to justify a tentative regressive equation.

(2) May this corrective equation, drawn from one kind of data (lifted weights), be applied in other fields (vision, temperature)? We venture to do so for several reasons: (*a*) The figures [r, R, p.e. (est.)] for S_1 and S_2 (Table III) agree very well indeed; whence we see that an equation for one set of weight-limens may be used to predict for another set by the same observers. (*b*) The values for II*b* and VIII may be predicted almost as well when they are omitted from the equation as when they are included; we may then predict for other observers in the same kind of material. Our direct evidence stops here; but we have shown elsewhere (Art. XI and XIII: *4*) that a common mode of distribution $\Phi(\gamma)$ appears in widely differing material; by the same token we believe that the Lexian ratios of normality will be much the same in these unlike fields. Any predictive equation is always open

to modification as new evidence appears; but the one above may serve in our opinion as a first approximation.

(3) How does our procedure compare in *accuracy* with Thomson's; even though his be time-consuming, will it not give truer values? Vain hope! Thomson's formula, though in principle sound, is inexact for two reasons (Art. VI, *3*): because of the approximate p.e.-formula which he uses (omitting all powers above the first in a Taylor-series which underlies the development; *cf.* Art VI, *3*, 86 or *8*, 78); and still more because the Urban-weight, P, occurs many times in his equation. This weight itself implies that the underlying percentages constitute a normal ogive; the more they depart from normality the less reliable is P. The data worked by Thomson *e.g.* (from Urban: I, S_2) have no inversions of either first or second order and are presumably as good as the average person will deliver (Urban rates I neither among his best nor his poorest observers: *13*, 284); and yet we have been able to prove that Urban's P for this material are in the mean but 76 per cent. of their true value. Consider the comparative figures for I, S_2:

Thomson's procedure for p.e.(M)	.151
The h-formula, taking ΣP_p as unity	.165
The h-formula, giving ΣP_p its true value (1.4895), but with no correction for overnormal dispersion (L)	.137
The h-formula, giving ΣP_p its true value and correcting for L by regressive equation	.266
True value [11] [p.e.(M) of nine fractional limens]	.259

Even when ΣPp is taken as unity and when uncorrected for overnormality, the h-values are as good as Thomson's (.165 or .137 as compared with .151); and when treated by the corrective equation are much better, being nearly correct (.266 vs. .259). The inadequacies above noted in Thomson's formula come to the surface in these figures; and exemplify our conclusion that his method has no discernible advantage to outweigh the labor which it entails. Either procedure is truly trustworthy only when the fitted percentages differ but little from the observed. So long as a curve is sensibly normal, either method is substantially correct; as the underlying

[11] Thomson, dividing by $n - 1$ (= 8) instead of n (= 9) gets .276 for p.e.(M).

percentages depart from normality, both are increasingly unreliable (insofar as our *h*-value is not corrected by the regression-formula); but the *h*-method is better in simplicity and speed.

Summary of Procedure

1. When the number of comparative stimuli (J) is less than five, or a rough measure of dispersion will do, the earlier formula (Art. VI, 3), which treats ΣPp as unity, may be used:

$$\text{p.e. } (Md) = \frac{.84535}{h\sqrt{2n}}.$$

With four (and even five) J, ΣPp is not far, as a rule, from one.

2. When the number of J is five or more, and a more precise p.e. is needed, use the formula herein proposed. In Table I is shown how ΣPp is computed which here equals 1.4895; n (number of times each pair of stimuli is compared) = 450; N_H (whole number of reports, weighted, on which the limen is based) = 450 × 1.4895 = 670.28; h = .1361. Solving, p.e. (Md) = .172, which is the desired value.

3. When L = 1 (observers who hold to the same criterion throughout) no revision of this figure should be attempted; it cannot be improved by the corrective equation. If the dispersion is thought to be clearly supra- or infra-normal and corrective treatment seems to be indicated, the Lexian ratio must be computed for each comparative stimulus. Divide the material into five or more equal sections (having 150 reports on J_1, make 5 sets of 30) and compute L as above (p. 468). Find their mean (arithmetic); in our example:

J	84	88	92	96	100	104	108	Mean
L	0.95	1.17	1.26	1.97	2.24	0.89	1.01	1.356

The corrective equation yields

$$R = 2.6 \times 1.356 - 1.6 = 1.926,$$

which multiplied by p.e. (Md) gives the 'most probable' value

$$.172 \times 1.926 = .331,$$

the actual value being (Table II) .325.

4. In the above example n is constant, each J being compared with S 450 times; in case n varies from one J to another (as recommended in Art. IV: *3*), we proceed as follows. Suppose, in lieu of 450 trials with all of the seven J, we had these numbers in order:

150	300	450	600	450	300	150
1	2	3	4	3	2	1

as in tier 7, Table I. Multiplying 6 by 7 we have (col. 8) a total weight of 3.4027, whence the net area N_H becomes $3.4027 \times 150 = 510.40$, as vs. 670.28 before. We need also to weight our Lexian ratios in the same way, thus:

$$0.95 \times 1 = 0.95,$$
$$1.17 \times 2 = 2.34,$$

and so on, the mean being $24.46/16 = 1.529$ as vs. 1.356 above.

5. The whole procedure may fairly be called simple and short, save when L is needed for correction. In our opinion, L is worthy of computation only when it is known or believed to be substantially greater than one (unseasoned observers who are changing under practise); in all other cases ($L \lessgtr 1$) the h-figures will do just as well.[12] They may now and then be a little too high (stimulus-attitude); but that seldom does any harm.

6. The so-called 'stimulus-attitude,' finally, proves to be a form of Poisson mechanism with sub-normal dispersion; in this fact its true nature is made to appear and its value for testing the differential powers of the organism is re-affirmed.

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[12] In our material (Table II, col. 7, 8) 9 in 30 actual p.e. are smaller than the correlative h-values; but even so, these agree better with the true p.e. in five, and less well in four, cases than do figures 'corrected' by the L-equation. In our material at least nothing is gained by using the corrective equation on sub-normal dispersions.

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TYPES OF DEXTRALITY AMONG NORTH AMERICAN INDIANS

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A recent survey of the manual and ocular habits of some 1,500 individuals has revealed certain significant patterns of behavior, called for convenience 'dextrality-types.'[1] The patterns referred to concern the use of the hands in bimanual as well as in unimanual activities and also the fixation-habits of the eye and the placing of the right or left thumb outside when the hands are clasped with fingers interlocking. As regards unimanual and bimanual operations, three dextral and three sinistral types are discriminated following a suggestion made by Rife.[2] Rife's types of dextrality are determined by noting the preferred hand in unimanual activities such as throwing or writing and the relative position of the two hands in bimanual activities such as batting, sweeping, spading and the like. Handedness is determined in bimanual operations by the hand nearer the functioning end of the implement. If in both unimanual and bimanual activities the right (or left) hand is preferred, the type is labeled RRR (LLL). If in unimanual activities an individual is right-handed (or left-handed) and in bimanual activities left-handed (or right-handed) the formula is RLL (LRR); such a person often describes himself as ambidextral. If an individual is right-handed (or left-handed) for unimanual performances but with a divided preference for bimanual operations; that is, right-handed (or left-handed) for performances demanding much skill (batting) and left-handed (or right-

[1] This survey, the results of which have appeared in the *Amer. J. of Psychol.*, (1927, 38, 317–367) under the title *Types of dextrality and their implications*, and also the facts to be reported in the present article are part of a study of handedness made possible by a grant from the National Research Council through its committee on Scientific Problems of Human Migration.

[2] J. M. Rife, Types of Dextrality, *Psychol. Rev.*, 1922, 29, 474–480.

handed) for those requiring less skill (sweeping), he is labeled RRL (LLR).

To put it concretely, the typically RRR individual writes and throws right-handed, swings a baseball bat in the conventional right-handed way, and places the right hand below the left on the broom-stick in sweeping. The RLL individual writes and throws right-handed; but he bats over the left shoulder with the left hand above the right and in sweeping he has the left hand below the right. The RRL individual writes, throws and bats right-handed; but in sweeping he places the left hand below the right. Corresponding types are found among the left-handed. Such individuals may be LLL, LRR or LLR in type. Certain intermediate or transitional forms occur, as in the case of those individuals who shift the relative position of the hands in batting, sweeping or spading, or who have a very mild unimanual preference.

The survey, previously referred to, showed significant percentage differences existing in the frequency with which the RLL and RRL types of dextrality occurred in superior and inferior groups of men. The superior group which was composed largely of professional men, college men and public school boys gave a significantly larger percentage of the RLL or ambidextral type and a significantly smaller percentage of the RRL or mixed type than did the inferior group which was composed largely of subnormals, psychopaths, and inmates of penitentiaries. The groups of the left-handed were too small to use statistically; but there was some evidence that the common type of left-handedness among superior men is LRR while the common type among inferior men is LLR.

Significant sex differences were also found in the relative frequency with which the RLL and RRL types occurred when superior men were compared with superior women. The men gave a significantly larger proportion of RLL cases and a significantly smaller proportion of RRL cases. That is, women are more frequently of a mixed type of handedness than men are.

Significant percentage differences were also found for the amount of left-eyedness occurring in superior and inferior groups of men, the latter yielding the greater amount. Women also show a significantly greater amount of left-eyedness than superior men. Significant percentage differences were found for right- and left-thumbs-out in the case of the totals for right and left-handed men, more right-handed than left-handed clasping the hands right thumb out, when the fingers are interlaced.[3] It is probable that clasping the hands with the left thumb out is indicative of at least a slight degree of latent sinistrality, since Lutz has presented evidence that the manner of clasping the hands is a heritable trait.[4]

The extent to which patterns of dextrality are determined by inherited differences or are the outcome of habits socially conditioned is an open question, although there is some evidence that native factors are involved. Apparently the types of dextrality are, at least in the case of the women who have been tested, connected with variations in personal-

[3] J. E. Downey, Further observations on the manner of clasping the hands, *Amer. Naturalist*, 1926, 60, 387–391.

[4] F. E. Lutz, The inheritance of the manner of clasping the hands, *ibid.*, 1908, 42, 195.

ity. The pure dextral appears to be more rapid and expert in movement than the mixed type, less subject to speech disturbances and less introverted. The mixed type, especially if there is asymmetry of eye and hand, gives a higher Thorndike score and a higher score on verbal tests, shows some special disability in tests of spatial orientation, and is often highly introverted. Comparable records have not been obtained for men, except in the case of Thorndike scores where there is a sex difference; the unilateralized and ambidextral male appears to be superior to the mixed type.

The above summary suggests that a survey of racial differences in dextrality may have great value, particularly if there should be further evidence forthcoming that such differences are indices to traits of personality. The thoroughly objective character of such an investigation would add to its value as a method in a comparative study of races.

In the present paper I wish to report data collected from 256 North American Indians, 110 males and 146 females. The groups are small, are not homogeneous; and the observations were not made in a wholly satisfactory manner. Therefore the report is to be considered merely tentative, but planned to stimulate further investigation.

The Indians tested were one group each of Shoshones and Arapahoes in reservations in Wyoming, and small groups of Papagoes, Yaquis, Mayos, Pimas and Apaches in Arizona schools. The Wyoming Indians were tested by Professor Richard S. Uhrbrock of the University of Wyoming, or his assistant, and the Arizona Indians by Miss Winnie Wheeler, Principal of a grade school, Tucson, Arizona, or her assistant.

The handedness records from the Shoshone Indians were obtained from the Shoshone Indian School at Fort Washakie, Wyoming, in September 1925. The Government School is a boarding school. Food, lodging and clothing are provided for the children and class instruction for the first eight grades until the children have completed such instruction, or until they are eighteen years of age. In addition to formal school work, training is given in certain farm work, dairying and poultry. At the time of Mr. Uhrbrock's visit to the school in September, approximately 150 children were in attendance. These were in the lower three grades. The older children were still at home, helping with the harvest. A great many of the children in the school bear the name "Washakie." Some of the children are said to be of mixed parentage—white, negro and Indian.

The handedness records from the Arapahoe Indians were obtained from two Catholic Mission Schools on the Indian Reservation, with headquarters at Fort Washakie, Wyoming. The first mission, St. Michael's, is about four miles from the Shoshone Indian School at Fort Washakie. The second mission, St. Stephen's, is

about four-and-a-half miles north of Riverton, Wyoming. The mission schools are boarding schools, built and maintained by the Catholic Church. The secular instruction is under the supervision of the Superintendent of the Government School at Fort Washakie. Each mission cares for approximately one-hundred children. Instruction is given in formal school work, covering the first eight grades. Additional instruction in farming, dairying and poultry raising also is given. The Arapahoes were said to be of purer blood than the Shoshones, but much interrelated.

The handedness records from the Arizona Indians were obtained from the Tucson public schools, the San Xavier Mission School and the Presbyterian Indian School, south of Tucson. But few children in the first grade were tested. Many of the Indian girls had never used either a broom or a spade. In such cases the examiner observed which hand and foot were more easily used. The observations were checked several times. Miss Wheeler informs me that the Yaqui Indians are supposed to be purer in blood than the other tribes. Marriage with whites, she says, is contrary to the law of Arizona and there is little tendency for marriages between Indians and negroes.

In connection with the actual test, teachers or superintendents were questioned concerning the amount of left-handedness they had observed among their Indian pupils and the effort made to induce changes in manual habits in case of left-handedness.

The Superintendent of the Shoshone Reservation, Mr. Haase, stated that, to the best of his belief, there were no left-handed Indians. This was contradicted by Mr. Graham, the school superintendent, who cited one or two cases in the school. They reported further that no attempt was made to enforce right-handed writing, and no special instruction was given in the use of the broom, or how to hold a baseball bat.

The teachers of the Arapahoe children stated that all the children were right-handed and that no attempt has been made to stress the use of the right hand in writing, nor to teach the children to sweep or use a ball-bat in a particular manner.

In the Presbyterian Indian School near Tucson only one left-handed pupil had been noticed by the teachers. The teachers of the Mission School reported that only minor left-handed tendencies had been noted and these infrequently. The Arizona Indian Children (both boys and girls) had played baseball from the earliest age except at the Mission School where the girls did not play. Almost no left-handed batting occurred. The playground supervisor had never made any effort to change the manual habits in batting.

The data which were obtained for me by my collaborators included the sighting eye for which two tests were given, namely Wray's ring test, and the manuoptoscopic test.[5] For the latter test Parson's instrument was used.[6] The following manual habits were observed.

The hand used in such unimanual activities as writing, throwing and eating
The hand on top in using the broom
Whether batting is done right- or left-handed, *i.e.* over the right or left shoulder and the relative position of the hands in swinging the bat.
To determine the thumb-habit, the hands were clasped with the fingers alternating.
The spade foot was recorded and, in the case of the Arizona children, the spade hand also.

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[5] C. Quinan, Sinistrality in relation to high blood pressure and defects of speech, *Arch. Intern. Med.*, 1921, 27, 257.

[6] B. S. Parson, *Left-handedness*, 1924, 78 f.

The fixating eye was checked and rechecked for the Arizona Indians; but this was not done for the Wyoming Indians where the time spent in the collection of materials was much shorter. I am, in fact, somewhat dubious about the amount of left-eyedness reported for the latter group.

Tables I and II give a tabular summary of the percentages

TABLE I

HANDEDNESS FORMULÆ; COMPARATIVE PERCENTAGES

Whites and North American Indians (Male)

Formula	WHITES				INDIANS	
	Superior Group		Inferior Group			
	No.	% of Total	No.	% of Total	No.	% of Total
RRR	173	41.09	150	35.62	62	56.36
RRL	146	34.67	194	46.08	39	35.45
RLL	73	17.33	42	9.97	5	4.55
RLR	3	.71	11	2.61	2	1.81
Left-Handed	26	6.17	24	5.70	2	1.81
Total: all Formulæ	421	99.97	421	99.98	110	99.98

TABLE II

HANDEDNESS FORMULÆ; COMPARATIVE PERCENTAGES

Whites and North American Indians (Female)

Formula	WHITES				INDIANS	
	Superior Group		Inferior Group			
	No.	% of Total	No.	% of Total	No.	% of Total
RRR	110	36.66	47	32.41	89	60.95
RRL	154	51.33	79	54.48	43	29.45
RLL	26	8.66	8	5.51	10	6.84
RLR	0	0	1	.68	1	.68
Left-Handed	10	3.33	10	6.89	3	2.05
Total: all Formulæ	300	99.98	145	99.97	146	99.97

for each formula obtained from the different groups of Indians. The third letter of the handedness formula is determined solely by the hand placed nearer the sweeping end of the broom. The records for males and females are kept distinct. For comparison, percentages are also given for white groups of superior and inferior status.[7] Table III

TABLE III

RIGHT-EYEDNESS: FORMULÆ RRR + RRL + RLL

	Male			Female		
	No.	No. R-Eyed	% R-Eyed	No.	No. R-Eyed	% R-Eyed
Superior Whites	319	247	77.4	285	185	64.9
Inferior Whites	266	171	64.3	132	85	64.4
Indians	108	59	54.6	143	90	62.9

[7] A few Negroes and Mexicans were included in the inferior group.

presents the data for the dominant eye, and Table IV for the dominant thumb.

TABLE IV

RIGHT-THUMBED: FORMULÆ RRR + RRL + RLL

	Male			Female		
	No.	No. R-Thumb Out	% R-Thumb Out	No.	No. R-Thumb Out	% R-Thumb Out
Whites	589	302	51.3	384	214	55.7
Indians	108	62	57.4	143	85	59.4

In addition to the errors incident to the collection of data, a certain degree of crudeness in classification grows out of the fact that in some cases the broom-hand and the spade-hand are not the same. Probably the classificatory scheme should be refined and this group sifted out from the whole. In the present survey and in that made for whites this was not done. About 30 per cent. of whites report the shift. For the Indians I have records on the spade-hand only for the Arizona group. For the Indian boys the shift was 22.6 per cent., slightly in favor of the right hand; that is to say, if the spade-hand had been used to give the third letter of the formula, the RRR group would have enlarged slightly at the expense of the RRL group. With the Arizona girls (who were stated, however, to be definitely inexperienced in the use of the spade) there is a shift of 35.6 per cent., but very largely in favor of the left hand; *i.e.* if the third letter of the formula had been determined by the spade-hand, the RRL group would have increased greatly at the expense of the RRR.

According to Rife, spade-hand and spade-foot should *always* be in agreement. In my survey of the dextrality habits of American psychologists I found a discrepancy in some 14 per cent. of cases. This was cut to 4 per cent. by sending out a second questionary. It appears that in some cases spading, as distinct from shovelling, may be practically a unimanual operation, with the second hand used in a very subordinate fashion to steady the implement. In such cases the discrepancy is apparent rather than real.

The discrepancy between the hand reported as the spade-hand and the spade-foot ran high for both the Arizona groups, 21 per cent. for the Indian boys and 19.1 per cent. for the Indian girls. The right foot is given as the spade-foot in an overwhelming number of cases. Wyoming Indian boys give 9.4 per cent. discrepancy between spade-hand and -foot; Wyoming Indian girls 36.9 per cent.

I am quite unable to judge the significance of so great an amount of discrepancy. One would wish to re-check the records. Quite possibly the observations were insufficient. The greatest discrepancy is found among the little Indian girls on the Wyoming reservation and the least among the Indian boys. The latter were the more highly practiced in the use of the spade.

Which of the tabulated percentage differences that appeared promising were actually significant was determined by the following procedure. Squared standard errors were computed for the categories under consideration, using the formula $\sigma_p^2 = \frac{pq}{n}$; and then the standard error of the difference

between any two categories was computed by the formula $\sigma_{p-p_1} = \sqrt{\sigma_p^2 + \sigma_{p_1}^2}$. The Kelley-Wood table of the normal probability integral was used in estimating the chances that the differences are significant. Table V gives the results for the handedness formulæ.

TABLE V

CHANCES THAT PERCENTAGE DIFFERENCES ARE SIGNIFICANT FOR HANDEDNESS FORMULÆ

Formula	σd	Diff.	$\frac{\text{Diff.}}{\sigma d}$	Chances in 1,000 that Diff'ce is Sign'cant
BETWEEN SUPERIOR WHITES AND INDIANS (M)				
RRR	.0531	.1527	2.8757	997
RRL	.0511	.0078	.1526	560
RLL	.0655	.1278	1.9511	974
BETWEEN INFERIOR WHITES AND INDIANS (M)				
RRR	.0527	.2074	3.9354	Above 999
RRL	.0512	.1063	2.0761	981
BETWEEN SUPERIOR WHITES AND INDIANS (F)				
RRR	.0494	.2429	4.8360	Above 999
RRL	.0473	.2188	4.6257	" 999
BETWEEN INFERIOR WHITES AND INDIANS (F)				
RRR	.0565	.2854	5.0513	Above 999
RRL	.0556	.2503	4.5017	" 999

It appears that the chances are more than 997 in 1,000 that the percentage differences are significant for the relative frequency with which the formula RRR occurs in the case of both superior and inferior white men and Indian boys. The same significant difference also occurred in the case of superior and inferior white women and Indian girls. The inference is that the Indians are more strongly dextral in type than the whites are.

Significant differences in the case of the formula RRL were found as between superior and inferior white women and Indian girls, the whites being more often RRL in type. The same difference holds for inferior white men and Indian boys, but *not* for superior white men and Indian boys.

There is an excellent chance that with larger groups the RLL formula would be found to occur with significantly greater frequency in the case of superior white men than in the case of Indian boys.

These results must be interpreted in the light of the significant differences occurring among the whites. I may repeat my summary of these as follows. A significantly greater proportion of inferior than of superior men are RRL in type and a significantly greater proportion of superior than of inferior men RLL in type; furthermore a significantly greater proportion of superior women are RRL in type as compared with superior men and a significantly greater proportion of superior men than of superior women are RLL in type.

The Indian boys resemble the superior group of whites in the significantly less frequency with which the RRL formula occurs when a comparison is instituted with inferior whites. The Indian girls show the same tendency when compared with the white women. On the other hand, the Indian boys resemble the inferior whites rather than the superior in the relative infrequency with which the RLL formula occurs. In the long report on *Types of dextrality and their implications* (previously referred to), evidence has been cited that the RLL, or so-called ambidextral type, is a distinctly superior type, and to this report readers should turn who are interested in the implications of handedness types.

Sinistral types LLL, LRR, LLR, corresponding to the dextral types, occur; but the Indian groups are too small to give any insight into their relative frequency. For Indians, the five cases reported were classified as follows:

Boys		*Girls*	
LLL	1 (Mayo)	LRR	1 (a doubtful case, ½ Papago)
LLR	1 (Shoshone)	LRL	2 (both doubtful cases; one Yaqui, one Papago)

Among whites, a significantly greater proportion of men than of women belong to the LRR type.

The handedness formulæ suggest, in agreement with the teachers' reports, that Indians are more dextral than whites. The ocular tests gave different results. Only one significant percentage was found, namely, between superior white men and Indian boys where the chances were over 999 in 1,000 that the superior whites were more right-eyed than the Indian boys. It is possible, as has been suggested in the

earlier report, that the training superior men receive in sighting accounts for their high percentage of right-eyedness. To repeat, these records on the Indians were not obtained under thoroughly satisfactory conditions and the results summarized in Table III should not be accepted without further confirmation.

No significant percentage differences were found for the dominant thumb. The chances that the percentage difference between white men and Indian boys is significant is 880 in 1,000. The drift of the figures is in favor of the Indian being more right thumbed, and with bigger groups this might be demonstrated.

As a matter of record I wish to include in this report a tabular summary of the finger-print patterns obtained from 50 Arapahoe Indians (see Table VI). The group is a small

TABLE VI

FINGER-PRINT PATTERNS (PERCENTAGES)

50 Arapahoe Indians

(Table prepared by Richard S. Uhrbrock)

Fingers	Hands	Whorls	Loops			Arches
			Rad.	Uln.	S Total	
All	Both	47.60	3.60	44.20	47.80	4.60
	R	50.00	4.40	40.80	45.20	4.80
	L	45.20	2.80	47.60	50.40	4.40
1	Both	74.00	0.0	26.00	26.00	0.0
	R	80.00	0.0	20.00	20.00	0.0
	L	68.00	0.0	32.00	32.00	0.0
2	Both	38.00	13.00	37.00	50.00	12.00
	R	40.00	16.00	32.00	48.00	12.00
	L	36.00	10.00	42.00	52.00	12.00
3	Both	24.00	3.00	66.00	69.00	7.00
	R	24.00	4.00	64.00	68.00	8.00
	L	24.00	2.00	68.00	70.00	6.00
4	Both	72.00	2.00	24.00	26.00	2.00
	R	72.00	2.00	24.00	26.00	2.00
	L	72.00	2.00	24.00	26.00	2.00
5	Both	30.00	0.0	68.00	68.00	2.00
	R	34.00	0.0	64.00	64.00	2.00
	L	26.00	0.0	72.00	72.00	2.00

one, the sexes not separated, and there are no sub-classifications on the basis of dextral types. I believe it possible, however, to illustrate even with such meagre material the possibility of a connection between finger-print patterns and

dextrality, and to urge the need of extensive investigation of such a problematic relationship.

Racial differences are known to exist with respect to the relative frequency with which the three accepted finger-print patterns (whorls, loops, arches) occur. I quote briefly from Bonnevie with reference to only one pattern, namely the whorl.

"The percentages of whorls give an interesting picture in so far as this pattern-type in its occurrence seems to show characteristic racial differences. In the populations from Eastern Asia we find no great difference between the percentage of whorls (ca. 43–45 per cent.) and that of ulnar loops (ca. 48–50 per cent.), while in the English and Norwegian populations this difference is very great, the percentage of whorls being less than one-half of that of the ulnar loops (Norwegians: 25–61.1 per cent.). The populations from India, Nias and the Aino people, as well as those from Italy, represent a medium relation between whorls (32–39 per cent.) and ulnar loops (53–51 per cent.)." [8]

Table VI would seem to indicate that the North American Indians fall with the Eastern Asiatics so far as the percentage of whorls relative to percentage of loops is concerned.

Differences in the relative frequency of different patterns on the two hands also occur.[9] Bonnevie writes:

"The distribution of pattern-types between right and left hands also proves to be peculiar, the whorls being considerably more numerous on right hands (29.38 per cent. against 21.92 per cent. on left hands), while arches (6.92 per cent. to 7.88 per cent.) and especially also loops (63.70 per cent. to 70.20 per cent.) are more richly represented on left hands. This difference is, within the loops, due to ulna loops only (57.76 per cent. to 64.52 per cent.) while the occurrence of radial loops (5.94 per cent. to 5.68 per cent.) is practically the same on both sides."

Bonnevie's group is, so far as types of dextrality are concerned, miscellaneous. One wonders whether an increased number of whorls would be found if the tabulation were confined to pure dextrals? Is there any connection between the high percentage of RRR cases for North American Indians and the high percentage of whorls? It is impossible to answer this question at present.

Bonnevie has made a detailed comparison of the corresponding fingers on the two hands and has found some interesting differences. Her statistical results were, as I have

[8] K. Bonnevie, Studies on papillary patterns of human finger, *J. Genet.*, 1924, 15, 1–109.

[9] K. Bonnevie, Main results of a statistical investigation of finger-prints from 24,518 individuals, *Eugenics, Genet. & Family*, 1923, 1.

already stated, obtained from a heterogeneous group so far as manual habits are concerned. If finger-prints were available for large groups of unlike dextral types, one would like particularly to compare the patterns of pure dextrals (RRR, right-eyed, right-thumbed) with those of pure sinistrals (LLL, left-eyed, left-thumbed) and both of these with ambidextrals, RLL or LRR. To collect these facts for adequate groups will be no light task. A recent survey of a class of forty gave me only *two* pure dextrals!

In the absence of thoroughly satisfactory material, a crude classification of subjects into right-handed and left-handed may yield some valuable information. In my previous report I gave the results for a small group of 96 left-handed persons. In general, Bonnevie's comparison between right and left corresponding digits held true; but there were reversals of percentages on right and left fingers in three particulars. Some noteworthy changes in percentages also occurred; *e.g.*, an increase of about 4 per cent. of whorls on the left thumb. For details and for other forms of comparison, the reader is referred to the original article. One further observation is perhaps worth citing here, namely, that there is an increase in the percentage of whorls on the right thumb when the right thumb is out, in clasping the hands, over that found when the left thumb is out.

In conclusion, I believe that the present report, although tentative, is sufficiently positive in outcome to suggest a field for exploration of racial differences and a method of attack that deserves serious consideration.

THE DEPENDENCE OF LEARNING AND RECALL UPON PRIOR INTELLECTUAL ACTIVITIES

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Introduction

Statement of Problem.—In the experimental investigation of memory it is a common observation that learning and recall exhibit wide daily fluctuations, and it may be assumed that these differences are due in part to daily variations in the subject's physical conditions and mental attitudes that are incident to the day's work. In a previous experiment, the writer (9) attempted to test this hypothesis by exercising some degree of control over the bodily and mental attitudes with which subjects approached a given task by introducing prior to learning and to recall certain mental and physical warming-up exercises. The experiments here to be reported were designed to obtain further data on the general problem of the effect of prior activities upon learning and recall. More specifically stated, this investigation is concerned with the dependence of memorizing and recall upon prior intellectual activities. The general problem resolves itself into two main parts; (1) The effect upon learning of intellectual activities aroused immediately before learning; and (2) the effect upon recall of intellectual activities aroused at varying intervals prior to that recall.

Method and Procedure.—The purpose of the prior activities was to arouse in the subject a certain familiar and particular background of experience, which for convenience we may designate by such terms as apperceptive system, set, disposition or trend. Two methods were used. In the first, the subject was presented with a brief resumé of a given field of knowledge, required to read and study it for a given period of time, and to answer a series of questions concerning it. The resumé in each instance consisted of two single-

spaced typewritten pages, with about thirty questions. The purpose of the questions was to encourage a careful reading of the text of the resumé. The second method was of the nature of an associational test. The subject was presented with, and required to answer, a series of specific questions covering a limited field of knowledge; *e.g.*, What was the date of the Missouri Compromise? When did the Civil War begin? Who were the Abolitionists?

In the various experiments a variety of apperceptive systems or backgrounds of experience were aroused. These were: Civil War, Revolution, Colonial Period, Biblical, English Literature, American Literature, Mathematics, Athletics, Grammar and Geography.

The learning materials consisted of lists of words or phrases and each list bore distinctly the connotation of a certain background of knowledge or experience. For example, one list consisted of words or phrases possessing a distinctly Civil War flavor, such as Lincoln, Emancipation Proclamation, Appomattox, Secession. Other lists were composed of items which were related to the American Revolution, Colonial Period, Old Testament History, English Literature, Athletics or College Sports, Mathematics and Geography. Each list consisted of eighteen words or phrases and caution was taken not to use words that appeared in the resumé or in the associational test, or words that would be likely to appear in the answers to these tests.

The experiments were conducted as a group-test. The words were mimeographed on a sheet and placed face-down before the subject. When the signal was given the subject turned the sheet over and memorized as many of the words as possible in the time allotted. At the expiration of two or three minutes time was called and the subject wrote on the back of the sheet as many of the words as could be recalled, irrespective of the order of presentation. Two minutes were given for writing down the words which he was able to recall in the immediate test as well as in the subsequent recall.

When the prior activity preceding learning consisted of the association-test, three mimeographed sheets were clipped together and placed before the subject face-down. The first two contained questions constituting the association-test, while on the third was the memory material. When the resumé was used as the prior activity, each subject was provided with two sheets containing the resumé, a third with questions based upon the resumé, and a fourth containing the material to be memorized. Each subject was provided with the materials and required first to carry out instructions relating to the prior intellectual activity. After these tasks were performed, the subjects were asked at a given signal to turn to the sheet containing the material to be memorized and to learn as many of the words or phrases as possible in the time allotted. At the expiration of the allotted time, the subjects were asked to turn the sheet over and to write on the back as many of the words as they were able to remember. The memorized lists were scored in terms of the number of words or phrases correctly recalled. Further details relative to the procedure can best be stated in connection with each experimental situation.

The subjects participating in these experiments consisted, in the main, of [illegible]giate students taking general psychology. But a number of high school and gramm[illegible]r grade students also participated. Something over a thousand subjects participated in the

series of experiments, and over six thousand individual measures of learning and recall were obtained.

The Influence of Prior Activities upon Learning

The first general problem is concerned with the influence upon the learning process of intellectual activities introduced immediately preceding the event of learning.

The first experiment was preliminary in nature and was designed to answer three questions: (1) Will the arousal of an apperceptive system facilitate or retard the rate of subsequent learning? (2) Is the nature or degree of the effect a function of the nature of the relation between the apperceptive system aroused and the materials to be memorized? (3) Which of the two methods, resumé or association-test, arouses the more effective apperceptive trend or disposition?

There were four groups of subjects and all learned the same lists of words in the same order. There were two lists having a Colonial History connotation, which we have designated Col. 1 and Col. 2; two lists having a Revolution connotation, signified by Rev.; and two lists having a Civil War flavor, indicated by C. W. The groups differed only in respect to the prior condition. Group A was the normal group, with no prior intellectual activity preceding learning. Group B was given before each list a resumé test on Biblical material that was unrelated to the material memorized. In the case of Group C there was given before each list a resumé test that covered the same field of knowledge as that from which the memorial material was drawn. For example, prior to the learning of the Colonial words, there was given a Colonial history resumé, whereas a Revolution resumé and a Civil War resumé were given prior to the learning of Revolution and Civil War words respectively. Group D was given prior to the learning of each list an association-test covering the same field of knowledge as that from which the memory material was drawn.

From an examination of Table I it will be observed that four measures, besides the number of cases, are included for each group; namely, the average number of words learned, the percentage of words learned, the sigmas of the distributions and the sigmas of the means.

An answer to the first problem may be obtained by a comparison of the records of Group A with those of B, C, and D. There is no good indication that the arousal of an apperceptive system influences learning either beneficially or [illegible]imentally.

[illegible]e second problem is answered by a comparison of the records of Group B and C. The differences are quite

small, yet the averages of Group C are consistently lower than the averages of Group B. This fact gives some indication that the presence of an apperceptive trend which is closely related to the memory material is slightly detrimental to learning.

TABLE I

RESULTS OF PRELIMINARY EXPERIMENT, SHOWING INFLUENCE OF PRIOR ACTIVITIES UPON LEARNING

GROUP	TESTS						
	Col. 1	Col. 2	Rev. 1	Rev. 2	C. W. 1	C. W. 2	Total
Group A:							
Ave. No. Words	12.8	13.8	14.6	15.0	14.3	15.2	14.3
% of Words	71.2	76.8	81.2	83.5	79.3	84.3	79.4
Sig. of Dis.	2.62	2.38	2.14	2.15	2.02	2.84	2.45
Sig. of Mean	.45	.42	.43	.39	.33	.31	.18
No. of Cases	33.	32.	27.	31.	37.	35.	195.
Group B:							
Ave. No. Words	13.3	14.2	15.1	15.2	14.4	15.1	14.6
% of Words	73.8	87.0	83.9	84.3	79.7	83.8	81.1
Sig. of Dis.	2.88	2.76	2.84	2.16	2.26	1.92	2.59
Sig. of Mean	.47	.45	.46	.35	.38	.32	.18
No. of Cases	37.	38.	38.	38.	35.	35.	221.
Group C:							
Ave. No. Words	12.3	14.1	14.7	14.9	14.2	14.8	14.4
% of Words	68.5	78.4	82.0	83.1	78.7	82.3	79.8
Sig. of Dis.	2.12	2.30	2.48	2.09	2.82	2.30	2.47
Sig. of Mean	.39	.41	.45	.39	.52	.40	.18
No. of Cases	30.	32.	30.	29.	29.	33.	183.
Group D:							
Ave. No. Words	13.1	14.2	15.5	14.9	14.6	14.8	14.5
% of Words	72.9	78.6	86.2	82.9	81.2	82.5	80.4
Sig. of Dis.	2.88	2.63	2.17	1.99	2.61	2.59	2.61
Sig. of Mean	.45	.42	.34	.32	.40	.40	.16
No. of Cases	41.	40.	41.	39.	39.	41.	241.

A comparison of the records of Groups C and D gives an answer to the third question. The differences are too slight and inconsistent to be significant. Therefore we conclude that there is no evidence that one method of arousing a particular apperceptive set is superior to another.

The main series of experiments was devoted to the task of obtaining more data concerning the relative influence upon learning of a congruous and a non-congruous apperceptive system. Two groups were employed, an experimental and a control. Both groups learned the same lists of words in the

same order, and they differed only in respect to the nature of the prior intellectual activities. With the control-group, the apperceptive system and the memory material referred to two distinct and relatively unrelated fields of knowledge, while with the experimental group the two referred to the same field of knowledge.

In these experiments, only the associational method was employed for the arousal of the apperceptive system. The test was lengthened to sixty questions, so selected as to arouse a whole background of knowledge in considerable detail. For example, Civil War questions not only covered the period of the sectional struggle, but also events leading up to it. Likewise the Colonial and Revolution periods were covered in considerable detail.

In this series each group was tested for three lists of words, and the experiment was performed three times with different pairs of groups. One list, instead of two, for each period constituted the material to be memorized. A further attempt was made to select words having a distinctive meaning, characteristic of the period and of no other. The results are given in Table II and the measures of reliability in II*A*.

Again the comparative data give some indication that the arousal of an apperceptive system that is closely related to the memory material is slightly detrimental. The differences are small and none of them are statistically significant; and yet the experimental group gave smaller average scores in seven out of the nine comparisons.

From an analysis of the error-records, which are not given in the tables, it is evident that a difference in the apperceptive condition exerted no appreciable influence on the number of errors.

The Influence of Prior Activities upon Recall

The second general problem is concerned with the effect upon recall of intellectual activities aroused at varying intervals prior to that recall.

The first experiment was designed to answer three questions: (1) Is the arousal of an apperceptive set effective in influencing recall? (2) Is the degree or character of the influence a function of the relation of the apperceptive system to the memorial material? (3) Is the effectiveness of an apperceptive system in influencing recall dependent upon the age or school standing of the subjects?

TABLE II

RESULTS OF SECOND SERIES OF EXPERIMENTS, SHOWING INFLUENCE OF PRIOR ACTIVITIES UPON LEARNING

GROUP	TESTS			
	Col.	Rev.	C. W.	Total
1st Exp.				
Experimental:				
Ave. No. Words	14.5	14.5	16.7	15.3
% of Words	80.9	80.7	93.2	84.7
Sig. of Dis.	2.35	2.67	1.51	2.44
Sig. of Mean	.26	.35	.18	.18
No. of Cases	66	59	67	192
Control:				
Ave. No. Words	14.9	15.7	16.1	15.7
% of Words	83.1	87.6	89.1	86.9
Sig. of Dis.	2.65	2.01	1.88	2.25
Sig. of Mean	.34	.27	.26	.17
No. of Cases	59	53	52	164
2d Exp.				
Experimental:				
Ave. No. Words	12.5	13.6	15.1	13.7
% of Words	69.6	75.8	83.5	76.2
Sig. of Dis.	2.00	2.03	1.60	2.15
Sig. of Mean	.28	.28	.23	.17
No. of Cases	51	54	48	153
Control:				
Ave. No. Words	12.8	13.6	15.5	13.9
% of Words	71.0	75.9	86.0	77.5
Sig. of Dis.	2.76	2.56	1.88	2.66
Sig. of Mean	.45	.39	.31	.23
No. of Cases	38	43	37	118
3d Exp.				
Experimental:				
Ave. No. Words	12.2	13.3	14.9	13.4
% of Words	68.0	73.8	82.8	74.3
Sig. of Dis.	2.26	2.07	2.33	2.41
Sig. of Mean	.33	.32	.34	.21
No. of Cases	45	42	43	130
Control:				
Ave. No. Words	13.1	14.1	15.5	14.2
% of Words	72.6	78.1	86.0	78.9
Sig. of Dis.	2.38	2.07	1.75	2.30
Sig. of Mean	.39	.34	.29	.22
No. of Cases	36	37	37	110
Total Experimental:				
Ave. No. Words	13.3	13.8	15.7	14.2
% of Words	73.7	77.1	87.4	79.1
Sig. of Dis.	2.46	2.36	1.80	2.49
Sig. of Mean	.19	.19	.14	.11
No. of Cases	162	155	158	475
Total Control:				
Ave. No. Words	13.8	14.6	15.7	14.7
% of Words	76.8	81.2	87.3	81.8
Sig. of Dis.	2.82	2.39	1.88	2.45
Sig. of Mean	.24	.21	.17	.12
No. of Cases	133	133	126	392

TABLE II*A*

DIFFERENCES OF MEANS AND SIGMAS FOR THE DATA IN TABLE II

Conditions Compared	Tests	Dif. of M.	Sig. of Dif.
Exp.—Control, 1st Exp.	Col.	.4	.43
	Rev.	.3	.44
	C. W.	.6	.31
	Total	.4	.25
Exp.—Control 2d Exp.	Col.	.3	.53
	Rev.	.0	.49
	C. W.	.4	.39
	Total	.2	.27
Exp.—Control, 3d Exp.	Col.	.9	.51
	Rev.	.8	.47
	C. W.	.6	.44
	Total	.8	.30
Total Exp. and Total Control.	Col.	.5	.30
	Rev.	.8	.28
	C. W.	.0	.22
	Total	.5	.17

The results of this experiment are to be found in Table III and the measures of reliability in Table III*A*. From an inspection of the table it will be observed that three grades of subjects participated in this experiment; college students, high school students and grammar grade students. The first column, headed 'memory material,' indicates the various kinds of memorial material used. The second column designates the kind of prior intellectual activity.

In this experiment the material was invariably learned under the normal condition, and the apperceptive system was always aroused immediately preceding the act of recall. Thus, each of the seven different lists of words indicated in Table III (Revolution, Civil War, Biblical, English Literature, Mathematics, Geography and Athletics) was recalled under three variable conditions. With the first group of subjects, no apperceptive system was aroused. This condition is denoted in the table by "None." In the second group, the apperceptive system aroused covered the same field of knowledge as that from which the memory material was drawn. For example, both referred to the Revolutionary Period, the Civil War period, the field of English Literature, etc. This relation between the apperceptive system and the memorial material will hereafter be termed a congruous relation. With the third group of subjects, the apperceptive system aroused always referred to a field of knowledge quite distinct from that from which the memorial material was drawn. For example, a Biblical apperceptive system was aroused prior to the recall of a list of Revolution words, an English Literature system before recalling a list of Civil War words, etc. This relation will be termed a non-congruous relation. Between learning and recall, there was an interim of forty-eight hours in the case of the college groups and an

TABLE III

RESULTS OF FIRST EXPERIMENT

Mem. Mat.	Prior Act.	Learning Ave.	Sig. Dis.	Sig. M.	% of Err.	Cases	Recall Ave.	Sig. Dis.	Sig. M.	% of Err.	Cases	% R. of Amt. L.
					(College Students)							
Rev....	None.....	11.7	2.04	.28	.8	52	10.4	3.3	.47	4.9	48	88.8
	Rev.....	12.3	2.02	.31	1.2	42	9.0	3.2	.51	9.1	40	73.1
	Bib.....	12.5	2.01	.31	.7	43	11.3	2.6	.41	3.5	41	90.4
C. W..	None.....	13.7	2.2	.31	.1	50	11.7	3.05	.44	2.1	48	86.8
	C. W.....	13.7	2.2	.33	.0	43	10.5	3.6	.57	2.0	40	76.6
	E. Lit....	14.6	1.9	.29	.2	44	11.9	3.3	.50	2.6	43	84.6
Bib....	None.....	11.6	2.4	.37	1.0	42	10.2	3.9	.60	2.3	42	87.8
	Bib.....	11.8	2.09	.34	.7	37	8.1	2.9	.49	12.3	35	68.6
	Rev.....	11.8	2.0	.31	.2	41	7.5	3.2	.52	6.4	37	63.6
E. Lit..	None.....	13.7	2.3	.36	.2	45	12.6	3.8	.59	1.8	42	91.7
	E. Lit....	13.2	2.06	.30	.7	41	7.8	4.4	.69	13.9	41	58.9
	C. W.....	13.6	2.13	.34	.3	40	8.7	4.2	.68	5.9	38	63.9
Math..	None.....	11.2	2.14	.34	1.8	39	7.2	3.22	.54	11.8	35	64.8
	Math.....	11.7	2.19	.37	2.6	35	4.9	2.26	.39	15.4	32	41.7
	Athl.....	12.1	2.0	.37	.6	29	6.6	2.5	.46	11.5	29	54.4
Athl...	None.....	12.9	2.21	.35	1.7	39	8.5	4.6	.78	15.4	34	66.2
	Athl.....	12.6	2.04	.34	2.3	35	6.8	3.01	.53	19.6	32	54.0
	Math.....	12.4	2.02	.36	2.5	31	6.0	2.36	.42	14.3	31	48.1
Ave. Totals	None.....	12.3	2.38	.15	.8	44	10.7	3.84	.24	5.3	41	76.6
	Cong.....	12.6	2.22	.14	1.1	38	8.1	3.77	.25	10.9	36	61.6
	Incong....	12.5	2.26	.15	.7	38	9.1	4.15	.28	6.1	36	67.5
					(High School Students)							
C. W..	None.....	12.7	2.4	.38	.0	39	11.9	3.88	.62	.8	39	93.7
	C. W.....	12.6	2.24	.44	.6	26	11.2	3.68	.72	4.3	26	88.8
	Athl.....	12.9	2.76	.59	.0	23	11.7	3.16	.66	1.5	23	90.7
Rev....	None.....	11.9	2.22	.30	.8	54	9.6	3.86	.52	3.9	54	80.6
	Rev.....	11.3	1.92	.33	.0	32	8.5	3.14	.55	6.5	32	75.2
	Math.....	11.1	2.36	.48	1.1	24	9.5	3.96	.80	5.6	24	88.3
					(Grammar Grade Students)							
Rev....	None.....	9.4	1.74	.24	1.0	53	8.1	2.43	.33	6.9	53	86.1
	Rev.....	9.2	1.90	.27	1.8	47	5.5	2.27	.35	17.4	42	59.8
	C. W.....	9.8	1.60	.24	2.1	42	6.8	2.24	.36	14.7	39	69.4
Geog..	None.....	11.0	2.12	.40	0.3	28	10.5	3.12	.61	3.8	26	95.4
	Geog.....	11.3	2.44	.41	.2	36	6.7	2.76	.48	16.6	35	59.3
	Math. Op..	10.3	2.44	.40	.8	37	8.1	2.46	.41	2.9	35	78.6
Total Ave....	None.....	11.2	2.51	.19	.5	42	9.8	3.12	.23	4.0	43	87.5
	Cong.....	10.8	2.47	.21	.7	35	7.6	3.44	.29	12.4	34	70.4
	Incong....	10.8	2.54	.23	1.1	31	8.8	3.02	.27	6.8	30	81.5

interim of twenty-four hours with the high school and grammar grade subjects. The remainder of the column headings will be clear from previous descriptions. The learning records are inserted in the table in order to check the possibility that the differences in recall scores are due to group-differences in learning ability.

The Influence of Apperceptive Set upon Recall.—The first question may be answered by a comparison of the condition where no activity preceded recall with the conditions in which recall was preceded by an intellectual activity, either congruous or non-congruous with the memorial material. We observe that the normal groups (absence of an apperceptive system) gave the higher recall scores in eighteen out of the twenty comparisons. Also the total averages for both the college subjects and the grammar and high school subjects show a superiority in favor of the normal group.

Not only did the normal groups recall more words, but they also made fewer errors in fifteen out of the twenty comparisons.

The Relation of Apperceptive System and Memorial Material.—The second problem raises the question whether the degree or character of the influence of an apperceptive system is a function of its relation to the memory material. An answer to this question may be obtained by a comparison of the records of the congruous and non-congruous conditions. Out of the ten experimental situations, the groups representing the non-congruous relation gave the larger recall scores in eight cases. We observe that the groups representing the non-congruous relation recalled 5.9 per cent. more than the groups representing the congruous relation, in the case of the college students; and 11.1 per cent. more in the case of the high school and grammar grade students.

The error-records indicate the same tendency; namely, that the presence of an apperceptive system congruous with the memorial material exerts a greater inhibitive effect than does the arousal of a non-congruous apperceptive disposition. The groups representing the congruous relation made more errors in nine out of the ten experimental situations. The average per cent. of errors in the case of the college students observing, for the congruous and non-congruous conditions,

is 10.9 and 6.1 respectively. For the high school and grammar grade students, the average per cent. of errors is 12.4 and 6.8 respectively for the same conditions.

Age or School Standing of the Subjects.—That the influence of the prior intellectual activities is not a function of the age or school standing of the subjects taking part, is indicated by an analysis of the comparative data. The total averages indicate that the same relationship obtains for the three experimental conditions. In other words, for both ages of subjects the normal condition is the best, whereas the congruous condition is the poorest. This relationship holds for errors as well as for amounts recalled.

TABLE III*A*

DIFFERENCES OF MEANS AND SIGMAS OF DIFFERENCE FOR THE DATA IN TABLE III

Conditions Compared	Learning		Recall	
	Dif. of M.	Sig. Dif.	Dif. of M.	Sig. Dif.
	(College Students)			
Rev. Words:				
None-Rev.	.6	.42	1.4	.69
None-Bib.	.8	.42	.9	.62
Rev.-Bib.	.2	.44	1.3	.65
C. W. Words:				
None-C. W.	.0	.45	1.2	.72
None-E. Lit.	.9	.42	.2	.67
C. W.-E. Lit.	.9	.44	1.4	.76
Bib. Words:				
None-Bib.	.2	.50	2.1	.78
None-Rev.	.2	.48	3.7	.79
Bib.-Rev.	.0	.46	.6	.71
E. Lit. Words:				
None-E. Lit.	.5	.78	4.8	.91
None-C. W.	.1	.79	3.9	.90
C. W.-E. Lit.	.4	.71	.9	.96
Math. Words:				
None-Math.	.5	.50	2.3	.67
None-Ath.	.9	.50	.6	.71
Ath.-Math.	.4	.52	1.7	.60
Athl. Words:				
None-Ath.	.3	.48	1.7	.94
None-Math.	.5	.50	2.5	.88
Ath.-Math.	.2	.49	.8	.67

TABLE III*A*—CONTINUED

Totals:				
None-Congruous	.3	.18	2.6	.34
None-Non-Cong.	.2	.21	1.6	.37
Cong.-Non-Cong.	.1	.18	1.0	.37
(High School Students)				
C. W. Words:				
None-C. W.	.1	.58	.7	.94
None-Ath.	.2	.73	.2	.90
C. W.-Ath.	.3	.76	.5	.97
Rev. Words:				
None-Rev.	.6	.44	1.1	.76
None-Math.	.8	.57	.2	.95
Rev.-Math.	.2	.58	1.3	.97
(Grammar Grade Students)				
Rev. Words:				
None-Rev.	.2	.36	2.6	.48
None-C. W.	.4	.34	1.3	.49
Rev.-C. W.	.6	.36	1.3	.50
Geog. Words:				
None-Geog.	.3	.57	3.8	.77
None-Math. Op.	.7	.56	2.4	.73
Geog.-Math. Op.	1.0	.57	1.4	.63
Totals:				
None-Cong.	.4	.28	2.2	.37
None-Incong.	.4	.30	1.0	.35
Cong.-Incong.	.0	.31	1.2	.39

Table III*A* is introduced to give some notion concerning the reliability of the measures. The first column shows the conditions compared, while the next four columns indicate the differences of the means and the sigmas of the difference for the learning and recall conditions. It will be readily observed that, according to statistical criteria, no one of the learning scores is significantly different, the sigmas of the difference being almost as large as the differences of the means, or even larger in a number of cases. This fact is significant from our point of view, since the group-differences in learning do not account for the differences of the recall scores. But the same groups recalled under variable conditions, and the statistical treatment of the data indicates that the variable conditions were effective in influencing recall. If we take as the criterion of reliability a difference which is three times its sigma, it will be observed that thirteen

out of the thirty-five comparisons signify reliability. The average sigma scores signify that the differences between the normal and the congruous conditions show greatest reliability, while the differences between the congruous and non-congruous conditions show least reliability. Aside from these considerations, we call attention to the general consistency of the results which is an important indication of reliability, even though certain of the comparisons cannot, when taken singly, be considered as statistically significant.

Thus the following conclusions are justified from a consideration of the above results; (1) A consideration of percentages recalled and the percentages of error show that the arousal of an apperceptive system prior to recall influences that recall detrimentally. (2) The arousal prior to recall of an apperceptive disposition which is closely related to the memorial material has a greater inhibitive effect upon that recall than has the arousal of an apperceptive system which is less closely related to the memorial material. (3) The influence of the prior intellectual activities upon recall is not a function of the age or school standing of the subjects.

The second experiment involved two problems. In the first place, we were desirous of obtaining further facts regarding the inhibitive influence upon recall of a congruous apperceptive system. Secondly, we wished to ascertain whether this influence is a function of the time-interval between learning and recall. Inasmuch as the retentive strength of the material learned decreases with the length of the interval between learning and recall, our second problem may be stated as the relation between the influence of a congruous apperceptive system and the degree of retention.

Four groups of collegiate subjects were used in this experiment, two experimental and two control groups. There was an interval of forty-eight hours between learning and recall in the case of one of the experimental and one of the control-groups; while with the other two groups there was an interim of ninety-six hours between learning and recall. All groups learned Revolution words under the same conditions, *i.e.* with no controlled activity preceding learning. The control groups recalled with no intellectual activity prior to recall, whereas the experimental groups recalled with a Revolution association-test immediately preceding the test for recall.

The results of this experiment are summarized in Tables IV and IV*a*. The first column of Table IV indicates the kind of material memorized. In the second column is shown the kind of intellectual activity preceding recall, with the numbers in parentheses signifying the number of hours elapsing between learning and recall. The other symbols are the same as those described in previous experiments. In Table IV*A* are shown the conditions compared and the differences of the means and the sigmas of the difference for learning and recall.

TABLE IV

RESULTS OF SECOND EXPERIMENT

		Learning					Recall					
Mem. Mat.	Prior Act.	Ave.	Sig. Dis.	Sig. M.	% of Err.	Cases	Ave.	Sig. Dis.	Sig. M.	% of Err.	Cases	% R. of Amt. L.
Rev.	None (48)	12.9	2.13	.31	.2	41	11.6	3.01	.43	2.4	38	90.0
	Rev. (48)	12.5	1.82	.26	1.2	46	8.3	2.74	.39	10.5	45	66.4
	None (96)	12.8	2.21	.22	1.0	38	10.0	1.84	.19	5.3	32	78.1
	Rev. (96)	12.8	2.59	.26	.7	43	7.1	2.57	.26	3.4	38	55.5

TABLE IV*A*

DIFFERENCES OF MEANS AND SIGMAS OF DIFFERENCE FOR THE DATA IN TABLE IV

	Learning		Recall	
Conditions Compared	Dif. of M.	Sig. Dif.	Dif. of M.	Sig. Dif.
None (48)-Rev. (48)	.4	.40	3.3	.58
None (96)-Rev. (96)	.0	.34	2.9	.32

In the first place, it is significant to note that these data confirm our previous conclusion that the arousal of an apperceptive system congruous with the learning material exerts an inhibitive effect on recall. This fact is indicated by the percentages recalled for both time-intervals and by the percentages of error in the case of the forty-eight-hour groups. It will be observed that all groups learned practically the same amounts, a fact which indicates that the groups were well equated with respect to learning ability. But in recall the control-group with a forty-eight-hour interval between learning and recall retained an average of 11.6 words, or ninety per cent. of the amount learned, while the corresponding experimental-group recalled 8.3 words, or only sixty-six per cent. of the amount learned. Thus from the standpoint of amounts recalled, as well as of the number of

errors in the case of the forty-eight-hour groups, the arousal of a congruous apperceptive disposition serves to decrease the efficiency of recall.

With reference to our main problem as to the relation of the inhibitive effect to the degree of retention, our results are somewhat inconclusive. Stated in absolute terms of the number of words, the presence of the apperceptive system exerted a slightly greater effect with the shorter interval (3.3 to 2.9); but in relative or percentage terms the effect was the same for the two intervals. On the basis of errors, however, the apperceptive system increased the percentages of errors for the shorter interval and decreased it for the longer interval.

The third experiment was devoted specifically to the task of answering the question whether the influence of an apperceptive system upon recall is a function of the time-interval between the arousal of the apperceptive set and the recall.

The data for this problem are found in Table V. The table is divided into three parts. In parts (1) and (2) the relationship between the memorial material and the apperceptive system is congruous, whereas the relationship in part (3) is a non-congruous one. Column one states the kind of memorial material and the second column designates the temporal position of the arousal of the apperceptive set. With the first group, signified by "None," which represents the standard or control-group, no intellectual activity preceded either learning or recall. In the case of the other groups, the intellectual activity either preceded recall, preceded learning, or was introduced immediately after learning, the symbolizations of which will be clear from an inspection of the table. Between learning and recall there was an interim of forty-eight hours.

We may discuss the data from the standpoint of previous conclusions and from the standpoint of the specific problem. An analysis of the data from the standpoint of previous results permits of the following conclusions.

1. A congruous apperceptive system invariably decreased the percentages recalled and increased the percentages of error in nine of the ten cases. This inhibitive effect was exerted irrespective of the temporal interval between the arousal of the system and the act of recall. This fact confirms and extends our previous conclusion concerning the inhibitory effect of a congruous apperceptive system.

2. A non-congruous apperceptive system decreased the percentages recalled in three of the four cases, and increased the percentages of error in but one case. The data are inconclusive.

TABLE V

RESULTS OF THE THIRD EXPERIMENT

		LEARNING					RECALL					
Mem. Mat.	Pos. of Act.	Ave.	Sig. Dis.	Sig. M.	% of Err.	Cases	Ave.	Sig. Dis.	Sig. M.	% of Err.	Cases	% R. of Amt. L.
(1)				(Congruous Relation)								
Bib.	None	11.7	2.23	.35	.8	41	7.3	2.43	.41	4.3	36	62.4
	Prior R.	12.4	2.44	.41	.2	35	5.4	2.52	.44	12.7	33	43.5
	Prior L.	13.2	2.52	.42	1.3	36	6.9	2.33	.37	7.6	35	52.4
	After L.	13.3	2.37	.42	.2	32	7.6	2.37	.42	10.4	32	56.9
Athl.	None	12.7	2.07	.39	2.5	28	7.4	2.74	.52	13.6	27	56.7
	Prior R.	13.2	1.91	.36	1.3	28	5.7	2.69	.52	23.1	27	41.7
	Prior L.	11.2	2.54	.42	3.4	37	5.9	2.33	.40	17.6	34	46.5
	After L.	13.2	2.42	.33	3.5	27	6.8	3.49	.68	10.0	26	50.3
Total.	None	12.1	2.17	.26	1.5	35	7.4	2.55	.32	8.6	32	61.1
	Prior R.	12.7	2.22	.28	.7	32	5.5	2.57	.33	17.8	30	43.7
	Prior L.	12.1	2.59	.30	2.3	37	6.4	2.34	.28	12.4	35	52.9
	After L.	13.2	2.39	.31	1.3	30	7.2	2.95	.38	10.2	29	54.6
(2)				(Congruous Relation)								
Math.	None	11.2	2.14	.34	1.8	39	7.2	3.22	.50	11.8	35	64.8
	Prior R.	11.7	2.19	.37	2.6	35	4.9	2.26	.40	16.5	32	41.7
	After L.	10.9	2.26	.38	5.1	36	5.7	2.81	.47	18.0	35	52.3
Athl.	None	12.9	2.21	.35	1.7	39	8.5	4.60	.78	15.4	34	66.2
	Prior R.	12.6	2.04	.35	2.3	35	6.8	3.01	.52	19.6	32	54.0
	After L.	11.8	2.35	.38	2.2	37	6.3	2.98	.49	18.6	34	53.3
Total.	None	12.1	2.32	.26	1.7	39	7.8	3.27	.39	13.8	34	65.3
	Prior R.	12.2	1.97	.24	2.4	34	5.9	3.30	.32	18.3	32	48.2
	After L.	11.4	2.37	.28	3.6	34	6.0	2.88	.34	18.3	34	52.7
(3)				(Non-congruous Relation)								
Math.	None	11.2	2.14	.34	1.8	39	7.2	3.22	.50	11.8	35	64.8
	Prior R.	12.1	2.00	.37	.6	29	6.6	2.50	.46	11.5	29	54.4
	After L.	10.3	1.89	.32	1.0	48	7.3	2.65	.42	5.4	41	71.2
Athl.	None	12.9	2.21	.35	1.7	39	8.5	4.60	.78	15.4	34	66.2
	Prior R.	12.4	2.02	.36	2.5	31	6.7	2.36	.42	14.3	31	54.4
	After L.	11.4	2.11	.33	4.3	41	6.9	3.53	.57	20.3	38	60.9
Total.	None	12.1	2.32	.26	1.7	39	7.8	3.27	.39	13.8	34	65.3
	Prior R.	12.2	2.02	.26	1.6	30	6.7	2.46	.30	13.0	30	54.4
	After L.	10.8	1.89	.20	2.6	44	7.1	2.92	.39	11.4	39	66.0

TABLE V*A*

DIFFERENCES OF MEANS AND SIGMAS OF DIFFERENCE FOR THE DATA IN TABLE V

Conditions Compared	Learning		Recall	
	Dif. of M.	Sig. Dif.	Dif. of M.	Sig. Dif.
(1) *Congruous*	(Bible Words)			
None-Prior R.	.7	.54	1.9	.60
None-Prior L.	1.5	.55	.4	.55
None-After L.	1.6	.55	.3	.59
Prior R.-Prior L.	.8	.59	1.5	.48
Prior R.-After L.	.9	.59	1.2	.61
Prior L.-After L.	.1	.59	.7	.56
	(Athletics Words)			
None-Prior R.	.5	.53	1.7	.74
None-Prior L.	1.5	.57	1.5	.65
None-After L.	.5	.51	.6	.85
Prior R.-Prior L.	2.0	.55	.2	.65
Prior R.-After L.	.0	.49	1.1	.85
Prior L.-After L.	2.0	.53	.9	.79
	(Totals)			
None-Prior R.	.6	.38	1.9	.46
None-Prior L.	.0	.40	1.0	.42
None-After L.	1.1	.40	.2	.50
Prior R.-Prior L.	.6	.41	.9	.43
Prior R.-After L.	.5	.42	1.7	.50
Prior L.-After L.	1.1	.43	.8	.47
(2) *Congruous*	(Mathematics Words)			
None-Prior R.	.5	.50	2.3	.64
None-After L.	.3	.51	1.5	.52
Prior R.-After L.	.8	.53	.8	.62
	(Athletics Words)			
None-Prior R.	.3	.49	1.7	.93
None-After L.	1.1	.51	2.2	.92
Prior R.-After L.	.8	.51	.5	.71
	(Totals)			
None-Prior R.	.1	.37	1.9	.57
None-After L.	.7	.38	1.8	.51
Prior R.-After L.	.8	.37	.1	.53
(3) *Non-congruous*	(Mathematics Words)			
None-Prior R.	.9	.50	.6	.68
None-After L.	.9	.47	.1	.65
Prior R.-After L.	1.8	.49	.7	.62
	(Athletics Words)			
None-Prior R.	.5	.50	1.8	.89
None-After L.	1.5	.48	1.6	.97
Prior R.-After L.	1.0	.49	.2	.71
	(Totals)			
None-Prior R.	.1	.39	1.1	.50
None-After L.	1.3	.33	.7	.51
Prior R.-After L.	1.4	.33	.4	.46

3. The data again confirm our previous conclusion that a congruous apperceptive system exerts a greater inhibitory effect than does a non-congruous system.

In respect to our main problem of whether the influence of an apperceptive system upon recall is a function of the time-interval between the arousal of the apperceptive set and the recall, the following statements may be made.

1. A congruous system exerted the greatest inhibitory effect when introduced immediately preceding recall in three of the four comparisons in respect to both percentages of recall and percentages of error.

2. It also exerted a greater effect when introduced prior to the act of learning than immediately after learning in three of the four comparisons and in the total averages for both percentages of recall and percentages of error.

3. We thus have the anomalous result that a congruous system aroused prior to learning does not appreciably influence that learning (Section II) but does exert an inhibitory effect upon the recall of that material forty-eight hours later.

The reliability of the data is shown in Table V*A*. If we accept a difference which is three times its sigma, only eight of the thirty-six comparisons show satisfactory reliability, when taken singly. The consistency of the results, however, does indicate some degree of reliability.

Summary and Conclusions

The arousal of an apperceptive system is ineffective in influencing learning. There is some slight indication that the arousal of a congruous apperceptive system influences learning detrimentally, but the results are inconclusive. This result is in agreement with the results of the writer's previous investigation concerning the influence of prior mental and physical conditions upon learning. So far as the writer is aware, there is nothing in the literature bearing on this particular problem.

The arousal of an apperceptive system prior to recall, however, influences that recall detrimentally. Its influence is detrimental from the standpoint of both the percentages

of words recalled and the percentages of error. This influence was consistently manifested throughout the three experiments on recall.

The arousal prior to recall of an apperceptive disposition which is closely related to (congruous with) the material memorized has a greater inhibitive effect upon that recall than the arousal of an apperceptive system which is less closely related to (non-congruous with) the memorial material.

The influence of an apperceptive set upon recall is not a function of the age or school standing of the subjects.

With reference to the relation of the inhibitive effect of the apperceptive trend to the time-interval between learning and recall or to the degree of retention, the results are somewhat inconclusive; but they indicate that the influence of the apperceptive trend is not dependent upon the degree of retention.

Concerning the temporal position of the arousal of the apperceptive set, the data show quite unambiguously that the influence of the congruous intellectual activities is detrimental to recall, whether introduced prior to recall, prior to learning, or immediately following learning. The detrimental influence was greatest when the apperceptive set was introduced immediately preceding recall and the least detrimental when it followed learning.

The experiments on recall can best be considered with reference to the phenomenon of retroactive inhibition. Since the time of the pioneer investigations of Müller and Pilzecker (4) upon this phenomenon, there has developed considerable interest concerning the influence of various activities introduced between learning and recall. It is not our purpose to review this literature, since that has been done by other investigators. We merely wish to call attention to certain of the results of the various investigations upon this phenomenon which are pertinent to our study.

There is general agreement among all the investigators mentioned in the bibliography that the interpolation of certain kinds of activities between learning and recall has an inhibitive effect upon that recall. They report that rest

is the best condition. This fact is in agreement with our results that the normal condition, with no intellectual activities prior to recall, is the most favorable condition for effective recall.

One aspect of the problem of retroactive inhibition which has most frequently engaged the attention of investigators is concerned with the question whether the inhibitive influence of the interpolated activities is a function of its similarity with the learning material. Müller and Pilzecker (4) found no relationship between the similarity of the interpolated activity to the original learning-material and the amount of retroactive inhibition. Robinson (5) and Skaggs (6) assert that the degree of retroactive inhibition is a function of this similarity. The results of Webb (8) and Brockbank (1) suggest that a relationship exists between the similarity of motor problems and the susceptibility to retroactive inhibition of animal subjects. Our results favor the conclusion that the amount of retroactive effect is dependent upon the degree of connection (similarity) between the memorial material and the intellectual activities.

Robinson (5) states that there is little or no agreement among the criteria employed as to the relationship between the degree of learning and the relative amount of inhibition. We found that, in relative (percentage) terms of recall, the inhibitive influence of the apperceptive set is not dependent upon the degree of retention. On the basis of errors, however, the apperceptive system exerted the greatest effect for the stronger degree of retention and the least effect for the weaker degree.

Müller and Pilzecker (4), Heine (3) and Skaggs (6) found that the interpolated activity exerted the greatest detrimental influence when inserted immediately following learning. Robinson (5) concludes that the retroactive influence is independent of the temporal position of the interpolated work. Our results show that the greatest inhibitive influence occurs when the intellectual activities were aroused immediately preceding recall, and that the inhibitive effect is less when the apperceptive set followed learning than when it preceded learning.

Our procedure has differed in three respects from those who have investigated the phenomenon of retroactive inhibition. In the first place, we used a longer time-interval between learning and recall, and, secondly, the type of intellectual activity preceding recall was different. Also we introduced the intellectual activity prior to learning and tested its effect upon subsequent recall. The inhibitive character of such a condition has not been tested by other investigators, so far as we are aware.

Two theories, the theory of perseveration and the theory of transfer, have been suggested to explain retroactive inhibition. Müller and Pilzecker have proposed the hypothesis of perseveration, which states that engaging in another activity immediately after learning interferes with the stamping-in of the memorized material. We have obtained two results, the inhibitive influence upon subsequent recall of intellectual activities aroused prior to learning and the least inhibitive effect when aroused immediately after learning, which cannot be explained in terms of perseveration but which can be more readily explained in terms of transfer.

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